The SCIENTIFIC MONTHLY

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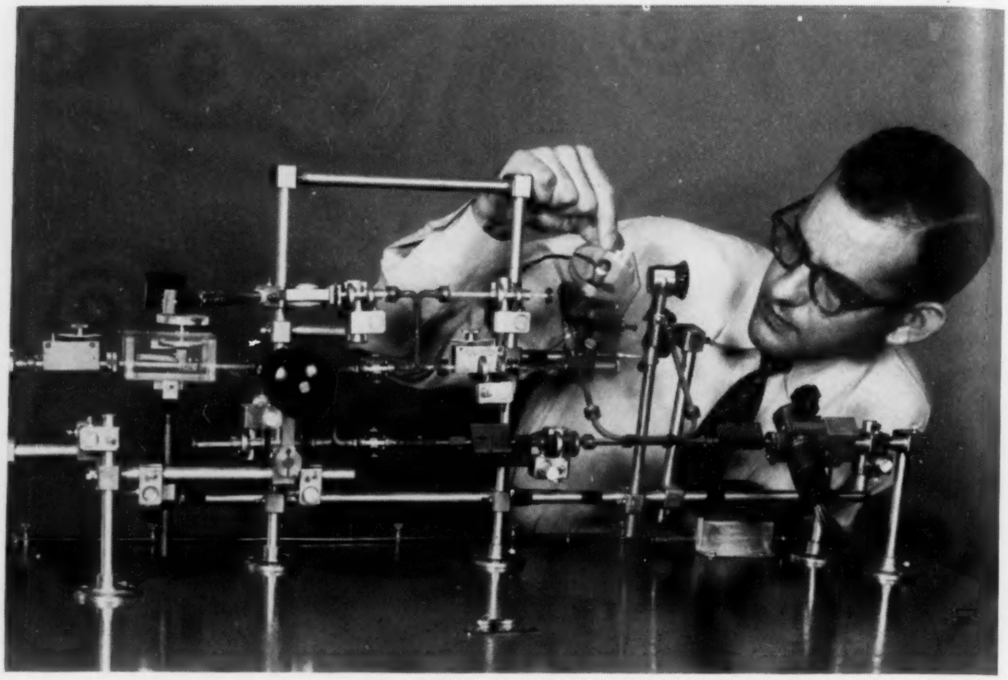
MAY 1956



Physicist G. K. Farney checks the frequency of Bell's new klystron, which is located at far right. Tube's output is about 20 milliwatts.

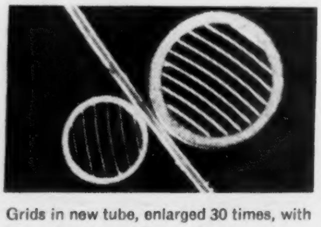
Sixty billion vibrations per second

A great new giant of communications—a waveguide system for carrying hundreds of thousands of voices at once, as well as television programs—is being investigated at Bell Telephone Laboratories.

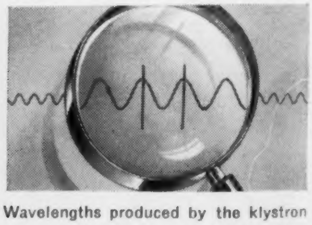
Such a revolutionary system calls for frequencies much higher than any now used in communications. These are provided by a new reflex klystron tube that oscillates at 60,000 megacycles, and produces waves only 5 mm. long.

The resonant cavity that determines the frequency is smaller than a pinhead. The grid through which the energizing electron beam is projected is only seven times as wide as a human hair, and the grid "wires" are of tungsten ribbon 3/10,000 inch in width.

G. K. Farney, University of Kentucky Ph.D. in nuclear physics, is one of the men who successfully executed the development of the klystron. Dr. Farney is a member of a team of Bell scientists whose goal is to harness the immense bandwidth available with millimeter waves . . . and to keep your telephone system the world's best.



Grids in new tube, enlarged 30 times, with human hair for comparison. Electronic beam passes through smaller, then larger, grid.



Wavelengths produced by the klystron tube are only .2 inch long—1/15 that of the transcontinental radio relay system.

BELL TELEPHONE LABORATORIES

WORLD CENTER OF COMMUNICATIONS RESEARCH

THE SCIENTIFIC MONTHLY

VOL. 82 | MAY 1956 | NO. 5

Cover: Partly eclipsed sun, Ceylon

[*Courtesy American Museum–Hayden Planetarium, New York, see page 228*]

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Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Drafting Aid

Panelgraph is a precision template with scaled outlines representing the most frequently used industrial instruments as well as various other symbols. It is fabricated from 20-gage frosted Lucite. (Panellit, Inc., Dept. SM, 7401 N. Hamlin Ave., Skokie, Ill.)

Adjustable Lead Shield

Atomlab adjustable lead shield model Als-1.5 is designed for use in making radioactivity measurements on low-activity samples and as a temporary storage for radioisotopes. The shield can be used to improve accuracy in counting problems by placing the Geiger or scintillation counter inside the cylindrical enclosure, which reduces the background count. The function and size of the shield may be varied by the addition or substitution of different sections. (Atomic Center, Inc., Dept. SM, 489 Fifth Ave., New York 17)

Drum Camera

Beckman and Whitley drum camera model 224, which is capable of transporting film at high velocities, offers flexibility in laboratory and field experimentation. As an oscilloscope camera, it has a range of writing speeds from 4 to 400 ft/sec; accessories permit its use as a streak camera or as a framing camera. A 50-in. circumference drum carries the film on its inside cylindrical surface, and a turret-mounted 45-deg first-surface mirror orients the optical axis perpendicular to the plane of the drum. When the drum is driven at its maximum speed of 6000 rev/min, the camera provides 10 msec of writing time on a 35mm by 50 in. film. (Beckman and Whitley, Dept. SM, 941 E. San Carlos Ave., San Carlos, Calif.)

Fluids and Lubricants

Ucon fluids and lubricants are described in a new booklet that covers properties, applications, and characteristics of various polyalkylene-glycol derivatives. Form 6500D. (Carbide and Carbon Chemicals Co., Dept. SM, 30 E. 42 St., New York 17)

Radioisotope Equipment

Berkeley division of Beckman Instruments has published a new catalog that describes radioisotope equipment for use in clinical medicine. Instruments used in medical radioisotope measurements, including gamma detectors, lead absorbers, scalers, meters, monitors, and handling equipment are described. Sketches of clinical applications of radioisotope equipment to diagnose thyroid function, localize brain tumors, or measure blood volume, survival time, and cardiac output are included. Catalog C-201. (Berkeley Div., Beckman Instruments, Inc., Dept. SM, 22 Wright Ave., Richmond 3, Calif.)

Power Supply

A regulated power supply suitable for calibrating meters and powering multistage amplifiers, computers, and various other laboratory instruments has been announced. Model PR 300 has an output tolerance for line voltage fluctuations of ± 10 percent of 0.002 percent or less. Reliability of voltage readings is 0.02 percent or 5 mv, whichever is greater. Other characteristics of the regulated power supply are its stability, low output voltage ripple, and low impedance. (Oregon Electronics, Dept. SM, 2232 E. Burnside St., Portland 15, Ore.)

Medical Motion Pictures

Magnetic sound camera with magnetic sound-on-film is a high-fidelity recording system that makes possible the filming of medical motion pictures with synchronized sound. The film passes through normal picture development and can be played back on any 16-mm magnetic sound projector. The Filmagnetic consists of a twin-head camera recording unit with record and instant-monitor magnet heads, a three-way input amplifier, high-fidelity microphone, complete cables, and self-contained batteries. (Berndt-Bach, Inc., Dept. SM, 6900 Romaine St., Hollywood 38, Calif.)

Chromatograph

Partitioner makes automatic qualitative and quantitative analyses of gases and volatile liquids by gas-liquid partition chromatography. The instrument has a 14-ft thermostatted chromatographic column to resolve each mixture into its components, and a built-in automatic integrator simultaneously computes and records a number proportional to the quantity of the component making the peak. The instrument is designed for identification of hard-to-differentiate trace impurities in solvents; on-the-spot analysis of fuels, flue and other plant-effluent gases, mine gases, air pollution, and smog; and quality control of foods, beverages, spices, tobacco, and perfumes. The partitioner, with 0.01- to 0.03-ml samples of liquid or 0.3- to 0.6-ml samples of gas, can be used to make an analysis of a multicomponent system in minutes, whereas distillation would require hours or days. (Fisher Scientific Co., Dept. SM, 418 Fisher Bldg., Pittsburgh 19, Pa.)

Angle Centrifuge

Refrigerated angle centrifuge has a maximum capacity of 2000 ml, a maximum speed of 15,500 rev/min, and direct motor-to-rotor drive. Seven different Servall rotors may be spun in the unit; maximum speed for large heads is 3500 rev/min; maximum speed for the medium heads is 5000 rev/min. Both the large and medium heads can be held at temperatures below 0°C. (Ivan Sorvall, Inc., Dept. SM, Norwalk, Conn.)

THE SCIENTIFIC MONTHLY

MAY 1956

Solar Eclipse Activities in Ceylon, 1955

THOMAS D. NICHOLSON

Mr. Nicholson is associate astronomer at the American Museum–Hayden Planetarium. He received his training at the U.S. Merchant Marine Academy, St. John's University, and Fordham University. From 1943 to 1945 he served aboard various vessels of the Moore-McCormack Steamship Company. In 1945 he joined the staff of the department of nautical sciences at the U.S. Merchant Marine Academy, where he remained until 1954.

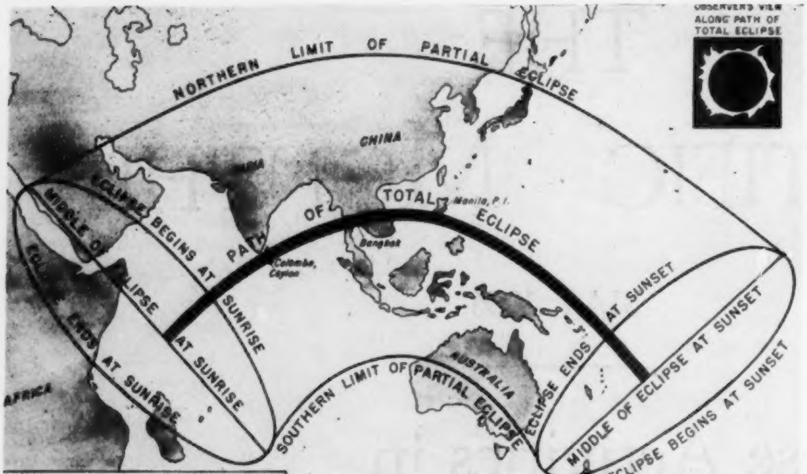
PREPARATIONS for observing the solar eclipse of 20 June 1955 began some 15 years ago. A. W. Mailvaganam, professor of physics at the University of Ceylon, began systematic weather observations in various parts of Ceylon, for the eventual purpose of selecting suitable locations for observation sites.

The remarkable feature of this eclipse was the duration of totality—it reached a maximum of slightly more than 7 minutes, 7.8 seconds at a point in the South China Sea. It was reported to be the longest eclipse in nearly 1250 years, and undoubtedly it was the longest to be the subject of modern astronomical investigation. It may have been the longest seen within recorded history, since the eclipse of A.D. 717, the most recent of longer duration, took place in landless areas of the South Pacific Ocean.

The eclipse map showed only three choices for observation: the island of Ceylon, a portion of the Malay Peninsula, and a belt through the Philippine Islands. In none of these areas, however, was there much hope of favorable weather during the month of June. In Ceylon, it is the season of the late spring and early summer monsoons, prevailing southwest winds carrying high temperatures and humidity from the Indian Ocean to the land regions of Southeast Asia. The relatively low and even lands in Malaya and the Philippines offered no shelter from the clouds and rain of the monsoons. Conditions in the Philippines were considered so poor that the staff of the Manila Observatory had given up all hopes of systematic observations as early as the spring of 1955, even though its location was within the belt of totality.

In Ceylon, however, the studies of Mailvaganam indicated a more optimistic outlook. A range of relatively high mountains centers around the 8300-foot Mount Pidurutalagala in the south central part of the island. In an area some 50 miles by 80 miles, the elevation exceeds 2000 feet above the coastal plains. To the west of these mountains, in and around Colombo, the June monsoons bring heavy rains and overcast, but to the east and northeast, because of the lee provided by the mountains, there is little or no precipitation at this time of year. Conditions on the two sides of the mountains differ so radically that each coast has a completely different economy and productive season.

In order to verify and reinforce the conclusions



Path of the solar eclipse of 20 June 1955. The few land areas crossed by the moon's shadow offered little opportunity for observing the longest solar eclipse in 1250 years. In parts of Ceylon, an 80-percent probability of clear skies was predicted. [Courtesy Hayden Planetarium]

of Mailvaganam, the Ceylon Department of Meteorology established four special stations on the eastern plains, near the centerline of the eclipse path, in addition to its permanent weather posts. Sunshine records for the months of June and July were obtained over a 4-year period. On the basis of their records, the director of the department was quoted, "The analysis of the sunshine data gives room for a lot of optimism; the chances of the sun being unobstructed by cloud during the eclipse is nearly 80 percent."

Guided by these and other favorable reports, astronomers from America, Europe, and Asia chose Ceylon as their location. The favorable weather reports and the duration of totality, exceeding 4 minutes, 30 seconds in Ceylon, attracted several large groups and seemed to warrant the costly transportation and installation expenses.

The staff at the American Museum–Hayden Planetarium became interested in the eclipse as an opportunity to undertake a project it had entertained for some time. We had reviewed an excellent educational film produced in the Soviet Union from materials photographed during the eclipse of 25 February 1952. It was distributed with commentary in several languages, including English. Based completely on the activities and equipment of Russian astronomers, it told the story of an eclipse and eclipse observations in a highly professional and yet entertaining manner, suitable for junior high school, college, or an adult public audience.

We wished to produce something similar, but an American film, featuring equipment and personnel of the Western World. To our knowledge, there was nothing of comparable quality available in American astronomy education films.

The eclipse of 1955 furnished us with an almost ideal situation for our purpose. It was an unusually interesting phenomenon because of the long period of totality. Preliminary reports indicated that there would be a great many astronomers, observing programs, and considerable equipment centered in a relatively small area of Ceylon. The locale from which the eclipse was to be studied was unique and strange by American standards, lending a touch of the dramatic by highlighting the long journeys astronomers must make to pursue the studies associated with such an event, the relative inaccessibility of eclipse observations, and the peculiar circumstances under which the longest of eclipses take place.

During the eclipse of 30 June 1954, we had familiarized ourselves with the techniques and equipment we would use. We were quite fortunate in a location in northern Michigan and obtained a fine series of color slides, which have since been included in the lending library of the American Museum of Natural History.

Our group, as with the other Americans on Ceylon, had the good fortune to obtain the cooperation of Trans World Airlines in accomplishing our objective. Under the instructions of the late Ralph S. Damon, president of T.W.A., an amateur astronomer, we received considerable logistic and administrative support from T.W.A. personnel and a generous grant to assist in defraying expenses. Damon was in Ceylon during the eclipse and visited most of the observing stations established there.

The American Museum–Hayden Planetarium party decided to locate somewhere on the east coast of Ceylon, somewhat south of the centerline, as offering slightly better statistical and theoretical op-

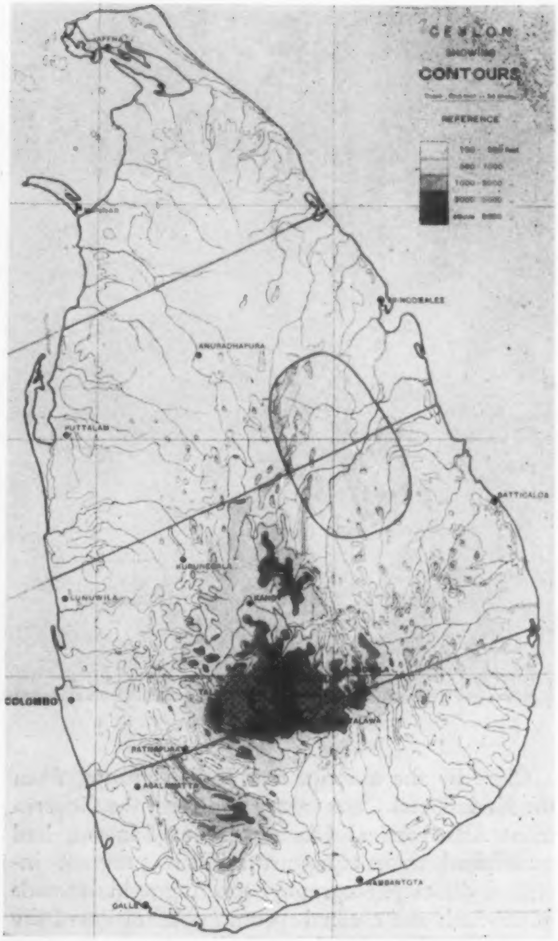
portunities for favorable weather. The mountains to windward were higher and the cloud cover generally less than they were at other locations.

Before setting up our own equipment for direct color motion picture photography of the eclipse, representatives from our team visited each major installation to film its equipment and activities, as an integral part of the story we intended to tell. As a result, we found ourselves fairly familiar with the complete program of activities taking place on that small island with respect to the eclipse.

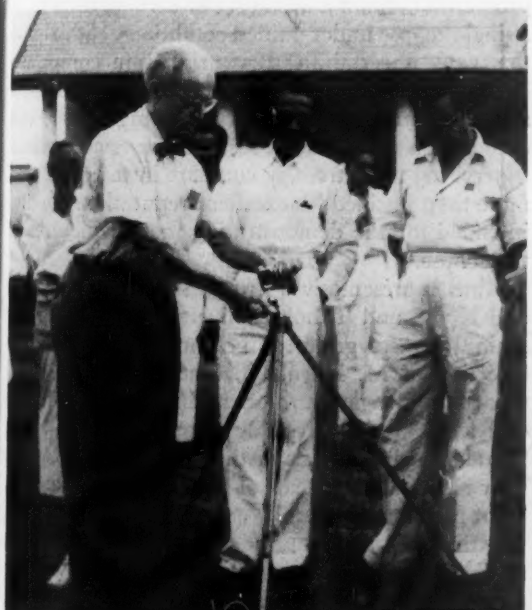
Most of the observing teams were within a short distance of the city of Polonnaruwa, almost exactly on the centerline, and on the eastern slopes of the mountain elevation. At nearby Hingurakgoda, an airport provided facilities for teams from Germany, England, France, Holland, India, and Ceylon.

Perhaps the most impressive installation was that of a group of astronomers from Potsdam, Cambridge, and St. Andrews. Some 15 tons of Zeiss equipment were transported from the Potsdam Observatory. Under the direction of H. von Klueber, of Cambridge, it was intended to remeasure the bending of starlight in the field near the sun, as predicted by Einstein. Von Klueber had worked with the same equipment in 1929, with results that were later the subject of some dispute. Two 28-foot telescopes were mounted horizontally, and images were projected into both from the same flat mirror. This would provide comparison photographs of the field near the sun and of another star field 90 degrees away. Any distortion introduced in the eclipse field by the flat could be determined by measurements in the comparison field. Assistance in this project was provided by students from the University of Ceylon. Although the star field around the sun was expected to be only moderately good, the duration of totality would compensate.

Also at the airstrip, A. Dollfus, Meudon Observatory, was using a Lyot interference filter for the hydrogen-alpha line. He planned to separate photographically the F-corona from the K-corona, since the hydrogen line appears in absorption in the former, but is absent in the emission spectrum of the latter.



Ceylon, showing the centerline and the northern and southern limits of the path of total eclipse. The shaded elevations in the south central region were expected to provide clear skies. Astronomers from 11 countries were located within the small curved area along the centerline to the east and north of the mountains. [Courtesy Hayden Planetarium]


R. Damon, president of T.W.A., at Batticaloa with officials of the local government. [Courtesy Hayden Planetarium]

J. Houtgast of Utrecht and his equipment for conducting studies of the spectrum of the solar chromosphere. [Courtesy Hayden Planetarium]

Close by the airstrip, two Indian groups, from the Kodaikanal Observatory and from the Government Astronomical Observatory at Banaras, had established their equipment. Their program included direct photography, spectrographic records of the flash and coronal spectra, polarization of the corona, and magnetic and ionospheric studies, under the direction of A. K. Das and Vainu Bappu. Das had also set up radio equipment for observations at a frequency of 200 megacycles per second.

J. Houtgast, of the Utrecht Observatory, had set up equipment south of the airfield for recording the spectrum of the chromosphere. Instead of relying on the popular jumping-film equipment, he used a broad diaphragm arrangement to limit the light of the corona and a narrow slit over part of the diaphragm to determine the level of the chromosphere being photographed.

Polonnaruwa is some 10 miles south and east of the airfield. It was once the center of a thriving Ceylonese civilization that reached a population of about 25 million at one time. Today the inhabitants of the island number about 8 million and Polonnaruwa is marked by stone and brick ruins of the old Singhalese temples, dating back nearly 1000 years. Two quite extensive expeditions, one from Zurich, Switzerland, and the other representing several Japanese observatories, camped here.

The Swiss team was under the leadership of M. Waldmeier, who is well-known for his investigations of coronal motion with the Lyot coronograph. His equipment included 18 instruments for a comprehensive study of the corona. He hoped by such a program to obtain a broad set of coronal data from a single eclipse under unified conditions, eliminating the many uncertainties that arise from comparing data obtained from different eclipses. Waldmeier, a somewhat impatient but very exacting leader, was executing a very difficult series of observations and supervising an extremely complex installation, and more than justified his excellent reputation. He also intended to take demonstration motion pictures of the eclipse.

Nine representatives from Tokyo Observatory and Kyoto and Tohoku universities made up the main Japanese group at Polonnaruwa, under the direction of M. Huruhata. They erected a small village on a sugar farm, including a machine shop and a darkroom. A smaller Japanese group was located on the campus of the University of Ceylon near Kandy, in the heart of the Ceylon tea country. That region is marked by extensive fog and rain at this time of year, but the geodetic and magnetic studies planned by the group would not have been disturbed by such conditions.

The Japanese, too, had planned a very comprehensive program of investigation into almost all phases of coronal research, as well as other

Horizontal telescopes set up by the Potsdam-Cambridge-St. Andrews group at Hingurakgoda to measure the Einstein effect. Inside the tent is the flat mirror for deflecting light into both telescopes. A. H. Jarrett of St. Andrews is in the foreground. [Courtesy Hayden Planetarium]

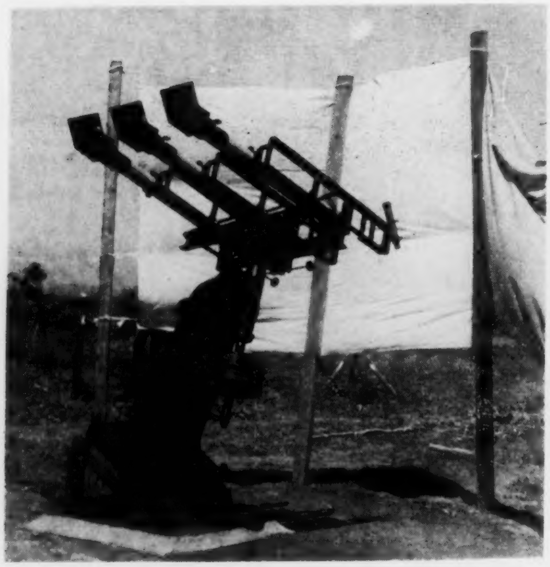
fields. The group from Tokyo Observatory had three objectives. They planned photoelectric studies of the far outer corona to measure the brightness between the outer corona and the zodiacal light, which is of importance in understanding the F-corona region and the distribution of particles in the vicinity of the sun. A 16.5-foot camera was mounted with a 12-inch coelostat for recording the spectrum of the outer chromosphere and the corona; four 4-inch cameras were equipped for polarization studies of the corona. It was planned, with these cameras, to derive contour lines of equal coronal brightness out to five solar radii and to make comparisons of the polarization in various areas.

A project similar to that of Dollfus was planned by the astronomers from Tohoku University—photography of the corona with a filter transmitting in very narrow spectral regions. Two astronomers from the department of astrophysics at Kyoto University were working with three motion picture cameras to photograph the flash spectrum. The cameras were mounted on separate telescopes on a single equatorial mount and were to photograph through filters in the blue-violet, red, and infrared regions.

On the same sugar farm, two British astronomers from the University of London were camped with equipment to photograph the coronal spectrum and to make polarization studies.

There were three American observing groups in Ceylon, one from Harvard University, located at Sigiriya, a second at Trincomalee, and the American Museum people. William Sinton was leader of the Harvard representatives, assisted by Owen Gingerich. They selected their location convenient to the Government Resthouse at Sigiriya, within short distance of the massive Sigiriya Rock, a huge almost-perfect cylinder of solid stone jutting up more than 500 feet from the rolling hills of the area. The rock had once been the fortress outpost of a warring Singhalese group which built a small city on its summit. They had carved a circular series of steps out of the stone, easily guarded as the single approach to their home.

Sinton had mounted two lenses of 36-inch focal length on a single fork mounting for making color measurements of the corona. He used six filters centered from 3540 to 14,970 angstroms, with pass bands of 200 angstroms. They were fixed to motor-driven wheels that automatically placed them in the focal plane in a fixed interval and sequence, each complete change taking 1 minute. One telescope was for the east-west and the other for the north-south sectors of the corona, at distances from 0.05 to 0.35 solar radii from the sun's edge. The brightness at each telescope was measured with phototransistors. Meter indications were to be read visually and recorded throughout the period of the eclipse by motion picture.



One of the Japanese instruments for making motion picture studies of the flash spectrum. Three cameras and telescopes were on a single mount. The tilt of the base compensated for the low latitude of Ceylon. [Courtesy Hayden Planetarium]

Harold Zirin, the third member of the Harvard team, had mounted a diffraction grating on the same frame and planned to photograph the complete infrared spectrum of the corona with film of various speeds and color ranges.

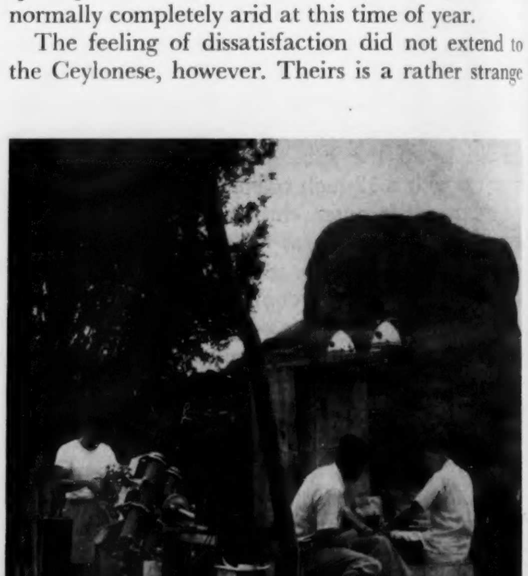
Another American team chose to locate at the British Naval installation at Trincomalee, on the east coast and somewhat north of the centerline. Arthur Adel, director of the Atmospheric Research Laboratory, and Allen Gardiner, Lowell Observatory, were making bolometric measurements of the radiation from the 9.6-micron band in the infrared spectrum of the ozone, at the zenith. They hoped to determine changes in concentration and temperature in the ozone layer during the eclipse.

Joseph Chamberlain, group leader, two American Museum photographers, and I made up the four-man Hayden Planetarium group. After completing our survey of the other eclipse sites, and obtaining picture records of the locale and general atmosphere of Ceylon, we proceeded to select an observing position on the east coast, in the southern portion of the eclipse path. We had initially chosen a beach resort at Kalkudah, but could not obtain accommodations there and found electric power difficult to get. We required electricity for the time-lapse and motor-driven equipment that we planned to use. For these reasons, we shifted to a location farther south, at Batticaloa, where we found rooms at the resthouse and all the facilities we desired.

Our principal purpose at this location was to obtain good quality color motion pictures with 1- to 6-inch lenses at various frame speeds, to stop the contacts, and to hold the entire eclipse in a continuous run. Other cameras and lenses were set up to obtain black-and-white and color still pictures with films of various speeds. A group of American Jesuits, stationed at nearby St. Michael's College, a Jesuit mission school, helped make our stay at Batticaloa a memorable one. Their assistance allowed us to expand the range of photography we had planned.

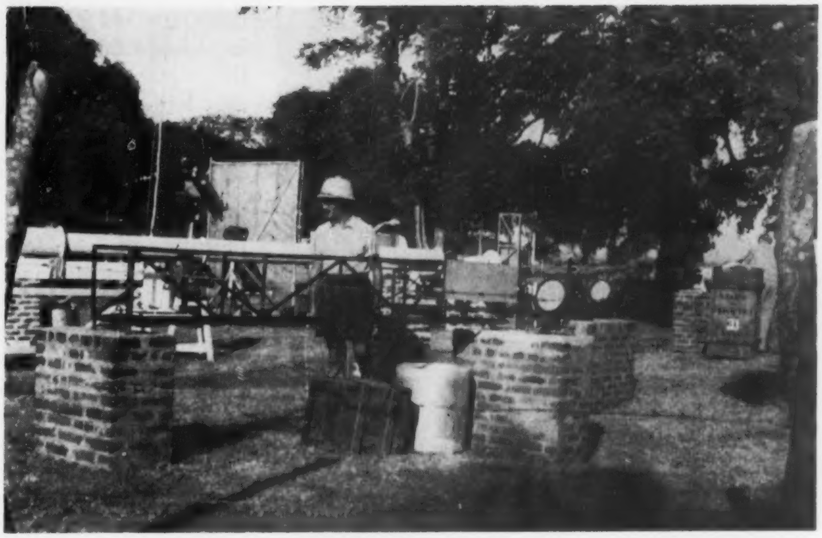
Several of the eclipse teams had been on location for months in advance, some of the Japanese for as long as 6 months and the Potsdam-Cambridge group for 3 months. All seemed dissatisfied with their locations as eclipse day approached. The prevailing weather pattern did not seem to justify the confidence of the Ceylonese authorities. Those on the eastern slopes of the mountains were bothered by generally excessive winds, which necessitated the construction of elaborate wind shields. A distinct feeling of pessimism could be detected in almost every observer as day after day passed with conditions that would be completely disappointing or questionable at best. The monsoons had evidently come rather late, and monsoon conditions were quite obviously extending over the mountains, even spilling occasional rains on the east coast, which is normally completely arid at this time of year.

The feeling of dissatisfaction did not extend to the Ceylonese, however. Theirs is a rather strange



Harvard camp near Sigiriya Rock. H. Zirin (left) with equipment for making photographs of the infrared spectrum of the corona. W. Sinton (center) and O. Gingerich (right) making recordings on the photometer. [Courtesy Hayden Planetarium]

Equipment set up at Polonnaruwa by M. Waldmeier and the Swiss team. In the left foreground are some of the coelostat piers. A spectrograph is mounted on the brick pier to the right. In the background is a horizontal telescope of long focal length. [Courtesy Hayden Planetarium]



personality. Long dominated by European masters —Portuguese, Dutch, and finally English—but now independent, they seemed to be so cooperative and so eager to render assistance that their assistance was sometimes unintentionally confusing. They would prefer to give you the answer you wanted, no matter how illogical, rather than to disappoint you with information more readily substantiated by the facts. To them there was no question but that the morning of 20 June would bring clear, unclouded skies and perfect observing conditions, simply because the astronomers wanted it that way.

It was interesting to note the attitude of the native Ceylonese toward the forthcoming eclipse. Considerable enthusiasm and interest were aroused throughout the island, but some doubt as well—doubt concerning whether or not it would be safe to watch the event. The Ceylonese are not ignorant people; the general level of education extends through the eleventh year. But even among the better educated there was an apparent sense of awe and uncertainty. Perhaps it was partly instilled by the poorly expressed warnings of the local newspapers.

Taking their cue from the obvious danger of looking directly at the sun with unguarded eyes, they reached the conclusion that it would be best if people did not watch the eclipse at all, disregarding well-smoked glass or exposed film as safeguards. Warnings to this effect were widely distributed, without adequate explanation of the reasons for the precautions. As a result, most people were convinced that there was some danger that would befall the island, and particularly the astronomers, who *had* to watch, at the time of the eclipse.

On top of all that, some of the local ayurvedic doctors—Ceylon recognizes two professional groups, doctors of medicine and ayurvedics, or herb doctors —had foolishly prescribed various potions to be taken during the eclipse. Some were to ward off the "evils" of the event, others to assure success in love, and still others to bring fertility to young women. Unfortunately, some of these potions proved to be toxic and thousands flocked to the government health stations and hospitals, jamming the roads in the hours following the eclipse.

In Batticaloa, where we were located, we had sought the cooperation of the chief of the local urban council, himself a licensed ayurvedic. Information provided to him and to other officials of the local government was widely disseminated, so that no such superstitions prevailed, and the day of the eclipse was anticipated with a healthy eagerness.

It is public knowledge, of course, that observation of the eclipse was a failure in Ceylon. On the morning of 20 June, three decks of clouds, at different heights and densities, and moving in different directions, provided an almost complete overcast. In only a few scattered locations did conditions become clear enough to expose the sun during totality —at the Colombo golf course, at nearby Sigiriya and the Harvard group, and at Kalkudah, where we had passed up our first choice of location. But even at these few places, conditions would not have been suitable for most of the studies being carried on. The reports are of almost complete failure. The few pictures taken during totality, mostly by local amateurs, showed a typical sunspot minimum corona but no detail. There were at least thin cirrus

At Batticaloa, an old wanderer stopped by to have a look at the sun. He was surprised to learn that no harm could befall him from watching the eclipse if he protected his eyes. [Courtesy Hayden Planetarium]

clouds even in the few scattered places from which the corona was visible. The partly eclipsed sun as it appeared through the clouds shortly after the third contact is shown in the photograph on the cover. The sun was not visible from Batticaloa during totality. In only a few isolated areas of Ceylon could the corona be seen at all.

Ironically, in the Philippines, which had generally been by-passed because of the unfavorable weather outlook, observers fared much better. In many areas, it was reported, the eclipse took place in perfectly clear skies.

Naturally, there was a great deal of disappointment among the astronomers on Ceylon. Even those doing work that could be successful with some cloud interference were not sure. Later, most of these reported failure too. Conditions were extreme, including rain and near flooding in some parts of the eclipse path. Waldmeier was so badly let down that he hauled the Swiss flag to half mast in disgust. Remembering the weeks and months of bad weather, and then the possibly worse conditions on eclipse day, we heard vague mutterings that perhaps the Ceylonese had not seen the sun in the mornings of June for decades but that they were just too polite and agreeable to say so.

But I doubt whether any of the visitors to Ceylon regretted their trip, provided, of course, that the expenses did not come from their own personal larder. It was certainly a pleasant and broadening experience, despite the strange food, primitive plumbing, and other departures from Western standards of living. Each of us will remember the cordiality and friendship, the willingness to help, of all whom we met.

As far as the American Museum group was concerned, the disappointment was not so great. We had obtained most of the materials we needed for our film project, and have already begun the process of editing and producing the motion picture story. That we failed to get the good quality and carefully timed motion pictures of the eclipsed sun was unfortunate, but for this we can provide substitutes. There will be other eclipses for us and also for the many astronomers on Ceylon who saw their carefully worked-out projects go down the drain. But we really felt sorry for the people in Ceylon. For most of them it would have been the one opportunity of a lifetime to see such a beautiful display in the skies. They lost it. They saw only a brief period of darkness come on during the day beneath a sky laden with clouds. But they did not seem to mind. Through it all, and after, they continued to smile and shake their heads in their peculiar manner. And they agreed among themselves, even as the astronomers had told them, that it was indeed a wonderful event.

Alchemy may be compared to the man who told his sons he had left them gold buried somewhere in his vineyard; where they by digging found no gold, but by turning up the mould, about the roots of their vines, produced a plentiful vintage. So the search and endeavors to make gold have brought many useful inventions and instructive experiments to light.—Francis Bacon.

Physics and Metaphysics

M. BORN

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THE subject which I have chosen to commemorate the great discoverer of the first law of thermodynamics has nothing to do with Joule's own work. In fact I would be quite incompetent to deal with experiments, and my knowledge of the history of Joule's discovery in connection with the work of his contemporaries, Robert Mayer and Helmholtz, is second-hand. I propose to speak about a very general matter. It is on the border line of two fields of research, and this seems to imply that I am familiar with both of them. However, though I feel on fairly stable ground when speaking about physics, I cannot claim in any way to be expert in what is customarily treated in philosophical books and lectures under the title metaphysics. What I know of it is more or less the recollection from my student days, refreshed by some sporadic reading. Long years of neglect have not deleted the deep impression received in my youth by the age-old attempts to answer the most urgent questions of the human mind: the questions about the ultimate meaning of existence, about the universe at large and our part in it, about life and death, truth and error, goodness and vice, God and eternity. But just as deep as this impression of the importance of the problems is the memory of the futility of the endeavor. There seemed to be no steady progress as we find in the special sciences, and like so many others, I turned my back to philosophy and found satisfaction in a restricted field where problems can actually be solved. Yet getting old, I feel, again like many others, whose productive powers are declining, the desire to summarize the results of the scientific search, in which, during several decades, I have taken a small part, and that leads unavoidably back to those eternal questions which go under the title of metaphysics.

Let me quote two definitions of metaphysics by modern philosophers. William James says, "Metaphysics is an unusually stubborn effort to think clearly." Bertrand Russell says, "Metaphysics, or the attempt to conceive the world as a whole by means of thought."

These formulations stress two important aspects; one the method: stubborn clear thinking; the other the object: the world as a whole. But is every case of stubborn clear thinking metaphysics? Every scientist, every historian, philologist, even theologian would claim to think clearly. On the other hand, the world as a whole is a subject not only vast, but definitely not closed, open to new discovery at any moment, therefore not exhausted and probably inexhaustible, in short the world known to us is never a whole. I shall return to this point at the end.

I propose to use the word *metaphysics* in a more modest way, in regard to method and subject as well, namely as an investigation of the general features of the structure of the world and of our methods to deal with this structure. I wish to discuss in particular the question whether the progress of physics has contributed anything essential to this problem. This progress of physics has been, as we are all aware, somewhat sensational during the last few years, and the aspect of the physical world has thoroughly changed in the half century of my own scientific life. Yet the methods of the physicist have always remained essentially the same: experimenting, observing regularities, formulating mathematical laws, predicting new phenomena with the help of these laws, combining the different empirical laws in coherent theories, which satisfy our sense of harmony and logical beauty, and testing these theories again by prediction. These successful predictions are the highlights of theoretical physics, as we have witnessed in our day in the case of de Broglie's waves, of Dirac's positron, Yukawa's meson, and many more such cases.

The power of prediction is the main claim of physics. It is based on the acceptance of the prin-

ciple of causality, which, in its most general form, means the assumption of invariable laws of nature. Yet you will all have heard that modern physics has been led to doubt this principle. Here is the first metaphysical concept on which I wish to make some comments.

Closely connected with it is the concept of reality. The skeptical attitude in regard to causality has arisen in atomic physics where the objects are not immediately accessible to our senses but only indirectly with the help of more or less complicated apparatus. These ultimate objects of physics are particles, forces, fields, and so forth; what kind of reality can one ascribe to them? This leads to the more general question of the relation between subject and object, of the existence of an objective physical world independent of the observing subject, and thus back to Russell's problem, whether a conception of the world as a whole is actually possible.

The cause-effect relation is used in ordinary life in two rather different ways, which may be illustrated by the following two statements:

"The capitalistic system is the cause of economic crises," and "the economic crisis of 1930 was caused by a panic at the New York exchange." One states a general rule or law, independent of time; the other declares one definite event to be the necessary sequence of another definite event. Both cases have the idea of necessity in common, a concept of a somewhat mysterious character which I feel completely unable to analyze further, and which I am willing to accept as metaphysical. Classical physics has officially adopted the second form of causality, as a necessary sequence in time. This came about through the discovery of the fundamental laws of mechanics by Galileo and Newton, laws which allow the prediction of future events from previous ones—or vice versa. In other words, these laws are deterministic: a world governed solely by them would be a gigantic machine; the complete knowledge of the situation at a given time would determine the situation at any other time. This kind of determinism was regarded by the physicists of the last century as the only rational interpretation of causality, and by using it they boasted that they had eliminated from physics the last remnants of metaphysical thinking.

Now it seems to me that this identification of determinism and causality is quite arbitrary and confusing. There are deterministic relations which are not causal; for instance, any time table or programmatic statement.

To take an absurdly obvious case, you could predict from the program of a pantomime the sequence of the scenes but would hardly say that the acrobats of scene No. 5 had caused the love scene No. 6. To return to science. The Ptolemaic system of the cosmos is a deterministic but not a causal interpretation, and the same can be said about Copernicus' cycles and Kepler's ellipses. They are all, in the usual scientific terminology, kinematic descriptions, but not causal explanations. For no cause of the phenomena is given except the ultimate cause of the creator's will. Then came the dynamical theories of Galileo and Newton. If one sticks to the program that the only aim of a theory is deterministic prediction, the progress made by the introduction of dynamics into astronomy could merely be seen in a considerable condensation and simplification of the laws. When I was a student in Germany, 50 years ago, this standpoint, skillfully formulated by Kirchhoff, was dominant and is still widely shared.

I think that the discovery of mechanics was a much more fundamental affair. Galileo showed that a certain quantity, connected with the motion of a body, namely its acceleration, is independent of the body and of its motion and only dependent on its position relative to the earth; and Newton showed the same for the planets where the acceleration depends only on the distance from the sun. This appears to me something more than a short and efficient description of facts. It means the introducing of a quantitative expression of the cause-effect relation in its most general form through the concept of force. It introduces the idea, foreign to the older kinematic theories, that one set of data (here positions) "causes" another set of data (here accelerations). The word *causes* means just "determines quantitatively," and the law of force expresses in detail how the effect depends on the cause.

This interpretation of the laws of mechanics brings them into line with the ordinary practice of the scientist. An experiment is planned, that is, certain conditions of observation are produced; then the effect is observed, sometimes at a future date, but more often all the time while the conditions hold. It is the timeless relation between observation and conditions of observation (apparatus) which is the real object of science. I suggest that this is the actual meaning of the principle of causality, as distinct from determinism, which is a special, and almost accidental property of the mechanical laws (due to the fact that one kind of the quantities involved are accelerations, that is, time derivatives).

If one looks on the history of physics during the last centuries from this point of view (as I have tried in my Waynflete lectures, which have been published under the title *Natural Philosophy of Cause and Chance* in Oxford) one gets the following impression:

Physics has used just this timeless cause-effect relation in its everyday practice but another notion in the theoretical interpretation. There causality was taken as synonymous with determinism, and as the deterministic form of the mechanical laws is an empirical fact, this interpretation was hailed as a great achievement, in eliminating dark metaphysical concepts. However, these concepts have a strange way of asserting themselves. Causality has in everyday life two attributes, which for shortness I shall call the principles of contiguity and of antecedence. The first states that things can act only on neighboring things, or through a chain of things in contact, and the second that if cause and effect refer to situations at different times the cause should be prior to the effect.

Both principles are violated by Newtonian mechanics, as the gravitational force acts over any distance of empty space, and as the laws of motion connect two configurations at different times in a perfectly symmetric and reversible way. One can regard the whole development of classical physics as a struggle to reestablish these two essential features of the concepts of cause and effect. The methods to preserve contiguity were mathematically developed, by Cauchy and others, by extending mechanics to continuous media; the idea of contiguity played a leading part in Faraday's researches in electricity and magnetism, and led to Maxwell's concept of a field of force propagating itself with finite velocity, which was soon confirmed by Hertz' discovery of electromagnetic waves. Finally Newton's case was brought into line with contiguity through Einstein's relativistic theory of the gravitational field. No modern theory of interaction is thinkable which violates this principle.

Antecedence has a much more tortuous history and not a happy end. It took much effort to discover that in physics the distinction between past and future was linked with the irreversibility of heat phenomena—here we remember Joule as one of the central figures—and to reconcile this result with the reversibility of mechanics through the development of atomistics and statistical methods. I think that this work, initiated by Maxwell, Boltzmann, Gibbs, and Einstein, is one of the greatest achievements in science. The deterministic interpretation of causality could be maintained for the atomic world and yet the apparent validity of antecedence understood as an effect of the statistical law of great numbers. However, this interpretation carried the gem of self-destruction of one of its pillars: it opened the way to the study of the atomic world, and the result was that the presupposed validity of Newtonian mechanics in this microscopic world was wrong. The new quantum mechanics does not allow a deterministic interpretation, and since classical physics has identified causality with determinism, the doom of causal explanation of nature seems to have come.

I am much opposed to this view. It does not matter much in discussions between scientists who know exactly what they are talking about; but it is harmful if used in describing the last results of science to the nonscientific world. Extremes are always harmful. The deterministic mechanistic view produced a philosophy which shut its eyes against the most obvious facts of experience; but a philosophy which rejects not only determinism, but causation, altogether seems to me just as absurd. I think that there exists a reasonable definition of the cause-effect relation which I have already mentioned: that a certain situation depends on another one (irrespective of time) in a way describable by quantitative laws.

I shall indicate how this is still true in quantum mechanics in spite of its indeterministic character, and how the apparent loss is compensated by another fundamental principle, called complementarity, which will be of great philosophical and practical importance.

This new concept is owing to Niels Bohr, the great Danish physicist, who was one of the leaders in the development of quantum mechanics, not only in regard to physics itself, but also to the philosophical implications. I was fortunate enough to listen to his Gifford lectures, given in Edinburgh some years ago. I cannot give you an account of his ideas in the short time left to me, but only try to outline the main points and to bring them into line with my slightly different formulations.

As you will know, Planck's fundamental law of quantum theory connects an energy E with a frequency ν, by the simple formula

$$E = h\nu$$

where h is a constant. This was later extended by Einstein and de Broglie, from the number of vibrations ν per unit time to the number of waves κ per unit length, which is connected with a mechanical momentum p by the corresponding formula

$$p = h\kappa$$

with the same constant h.

That this is so, has been confirmed by innumerable direct experiments and more or less indirect inferences from observations. Whenever a process can be resolved into periodic components with definite periods in time and space, that is, with definite ν and κ, the effect of it on the motion of particles consists in transferring energy and momentum according to this law. This empirical fact

must be accepted as undeniable before its implications can be discussed.

Now this fact is so extremely strange that it took many years before physicists began to consider it seriously, and Niels Bohr himself has used the word *irrational* to describe the new feature of the physical world discovered by Planck. Why irrational? Because energy and momentum of a particle are, by their definition, related to an extremely small region of space, practically to a point, while frequency and wave number also by their definition, are related to a very large, theoretically infinite, extension of space and time. This latter point will perhaps not appear so obvious as the first; you may say, I hear a tone of a piano string well defined even if it is played extremely staccato. This is practically true, because our ear is not a very sensitive instrument to discover tiny distortions. But the telecommunication engineer is familiar with the fact that there is a distortion. A tone lasting only a short time, comparable with the period, is not pure any more but accompanied by other tones, with frequencies spread out over a little interval $\Delta\nu$ around the original one; and if the duration is getting shorter and shorter this interval becomes larger and larger, until no tone is heard, but a noise, a crack. As modern telecommunication is based on the principle of modulation, that is, of interrupting a high-frequency current in the rhythm of signals or modifying its strength according to the relatively slow vibrations of speech or music, it is obvious that there is a limit to the perfection of transmission: if Δt is the duration of a tone of frequency ν, there is a relative limit of recognizability given in order of magnitude by

$$\Delta t \cdot \Delta \nu \sim 1$$

An excellent account of these problems has been given by Dr. Gabor in England, and in America a book with the title *Cybernetics,* that is, the science of governing—namely by sending out signals and orders—has been published by Norbert Wiener which, though full of rather abstruse mathematics, has made quite a sensational stir. In fact the mathematical analysis of these relations which has its roots, almost one and a half centuries ago, in an investigation by Fourier on the conduction of heat, is rather simple. The main point is that the ideal, or pure, or harmonic vibration to which alone a sharp frequency can be ascribed, appears in a time-amplitude diagram as an endless train of sinusoidal waves. Every other curve, for instance a wave restricted to a finite interval of time is a superposition of harmonic waves and has a whole "spectrum" of ν-values. The same holds for real waves expanding in space where apart from the periodicity in time one has a periodicity in space measured by the wave number κ; between the length Δl of a train of waves and the width $\Delta\kappa$ of the κ-spectrum one has the relation

$$\Delta l \cdot \Delta \kappa \sim 1$$

There is no other logical way of dealing with periodic processes or waves than this Fourier analysis, and practical applications have amply confirmed the theory.

Let us return to quantum physics. The "irrationality" can now be formulated more precisely; in order to define ν and κ sharply, one has to have very small $\Delta\nu$ and $\Delta\kappa$, hence a very long duration

$$\Delta t \sim 1/\Delta \nu$$

and spatial extension

$$\Delta l \sim 1/\Delta \kappa.$$

So far nothing is different from the case of telecommunication, and nothing paradoxical. But if one uses the relations $E = h\nu$, $p = h\kappa$ and rewrites the limiting relations in the form

$$\Delta t \cdot \Delta E \sim h, \Delta l \cdot \Delta p \sim h$$

they indicate a paradoxical situation: that with a tiny particle of sharp energy and momentum (that is small ΔE and Δp) there are associated long intervals of time and space Δt and Δl. What can the meaning of Δt and Δl be?

The only possible answer is, that they mean the limits for determining the position of the particle in time and space. They are indeed nothing but Heisenberg's much discussed uncertainty relations.

Thus is is seen that the very first quantum laws lead necessarily to a mutual restriction in the accuracy attainable in space-time location on the one hand, energy-momentum determination on the other. As Bohr has stressed again and again, we are confronted here with a logical alternative: either to deny the validity of an enormous amount of experience confirming the quantum laws $E = h\nu$, $p = h\kappa$, or to accept the existence of those limits for the determination of such pairs of quantities, as time-energy, coordinate-momentum, which in the mechanical terminology are called conjugate. The most remarkable thing is that in spite of the completely new and revolutionary basic situation it was possible to develop a quantum mechanics which is a straightforward generalization of classical mechanics, extremely similar in mathematical form and considerably more perfect in its structure. It is true that the simple way of describing variable quantities as functions of time has to be given up and a more abstract method introduced where physical quantities are represented by noncommuting symbols (that is, symbols with which one

can form sums and products; but the value of the latter depends on the order of the factors).

I shall never forget the thrill which I experienced when I succeeded in condensing Heisenberg's ideas on quantum conditions in the mysterious equation

$$pq - qp = h/2\pi i,$$

which is the center of the new mechanics and was later found to imply the uncertainty relations.

The transition from the symbols to actual quantities which can be measured is made by the introduction of a quantity called wave function, which describes the state in which a system is found as far as it can be described: its square is the probability density for finding the given data (for example, coordinates of particles) in a given small region, analogous to the distribution function of ordinary statistics. There is, however, a fundamental difference.

Suppose two beams of particles coming from the same source counted separately, give the results ψ_1^2 and ψ_2^2; if by a suitable arrangement they can be made to overlap and be counted together, the result is

$$(\psi_1 + \psi_2)^2,$$

which differs from the sum

$$\psi_1^2 + \psi_2^2$$

(by $2\psi_1\psi_2$). One has "interference" of probabilities, as well known from the case of light quanta or photons, the particles whose abundance is measured by the square of the intensity of an electromagnetic wave. But I cannot enter into a technical description of wave mechanics which has been developed from the foundations laid by de Broglie, through the ingenuity of Schrödinger, Dirac, and others. It suffices to say that a wave function ψ can be regarded as a packet of harmonic waves of different ν and κ, and that the physical quantities like coordinates, momenta, energies q, p, E, are operators distorting the ψ-function, and thus determining the strengths of the harmonic components of the packet from which by squaring the probability of the appearance of particles with given $E = h\nu$, $p = h\kappa$ is obtained.

Thus the new mechanics is essentially statistical and, in regard to the distribution of particles, completely indeterministic. Yet it preserves, strangely enough, some similarity to classical mechanics, as the law of propagation of the function ψ, the so-called Schrödinger equation, is of the same type as the wave equations of elasticity or electromagnetism. One has therefore the somewhat paradoxical situation, that there is no determinism for physical objects, like small particles, but for the probability of their appearance. Yet this determination of the ψ-function needs exteremely more data than those we are accustomed to in classical mechanics (initial positions and velocities of particles). In fact it needs a knowledge, or at least a hypothetical knowledge, of ψ everywhere at given time, and at the boundaries at all times, for the region and period in question; or in other words: predictions even of probabilities alone can be made only with reference to the whole situation, to the apparatus used. One must decide beforehand which feature one wishes to investigate, and one must construct the instrument correspondingly. Then the effect can be predicted, in terms of particles as a probability of their appearance under the conditions of the experiment (for example, with given momentum), either at a certain finite region independent of time, or at a later time. That is in complete harmony with the meaning of causality which I have suggested. The use of this terminology is not a mere decoration; for it is essential to be clear that here the metaphysical, irreducible concept of necessity in the relation of two sets of things is postulated, which is the characteristic feature of the scientific attitude to the world.

Summarizing we may say that while classical physics assumes natural phenomena going on independent of the incident of observation and describable without reference to observation, quantum physics claims only to describe and predict a phenomenon in relation to a well-defined mode of observation or instrumental arrangement. But one can, of course, use different instruments for observing the same class of phenomena; the propagation of light for instance can be investigated by prisms or grating with help of photographic plates or Geiger counters. If every arrangement, from the standpoint of quantum mechanics, has to be considered separately, what is the common feature of all of them? For instance, if by one arrangement we can determine the spatial distribution of electrons, by others their distribution in energy, how can we know if and when we have exhausted all possibilities?

This question has been discussed in detail by Niels Bohr under the title *Complementarity*. It is true that he presents his ideas in a little different way: he is keenly intent to show by simple examples how one can intuitively understand the wholeness of an experimental situation and the mutual exclusiveness and complementarity of two such situations by using nothing but the uncertainty principle in its simplest form. I think that his motive in spending much ingenuity and effort on this task is the tragic situation that the philosophical attitude accepted by him and presented here by myself, also accepted by the whole international community of atomic physics, has not found favor

in the eyes of just those men who have contributed most to the development of quantum theory, Planck and Einstein. Planck preserved always a cautious attitude to the revolutionary consequence of his own discovery, but Einstein went further and made repeated efforts to show by simple examples that the renunciation of determinism and the uncertainty relation are wrong. Just these examples have been studied by Bohr (in collaboration with Rosenfeld); in every case Einstein's objections could be refuted by a refined study of the experimental situation. The main point is that an instrument, by its very definition, is a physical system whose structure can be described in ordinary language and whose functioning in terms of classical mechanics. Indeed, this is the only way in which we can communicate about it with one another. For instance, any spatial location needs a rigid frame, any measurement of time a mechanical clockwork, while on the other hand a determination of momentum and energy needs a break of rigidity and mechanical connection, a freely movable part of the instrument to which the laws of conservation can be applied. Now Bohr shows that these two types of arrangement are mutually exclusive and complementary, in exact agreement with the results of the theory. If you use a diaphragm with a slit for fixing a coordinate of a particle passing through it, the diaphragm must be fixed to the frame of the instrument; if you wish to know whether a particle has really passed the slit, the diaphragm must be movable so as to be able to recoil. You cannot have it both ways. By taking this complementarity into account, one can describe experiments without contradictions.

You will find numerous examples in Bohr's Gifford lectures. While I wrote this, a new book came into my hands, *Albert Einstein, Philosopher and Scientist* (The Library of Living Philosophers, Editor Paul Arthur Schilpp, 1949), which contains articles of many philosophers and theoretical physicists on different aspects of Einstein's work, among them also one by Niels Bohr and one by myself. The most interesting part of the work is a scientific autobiography by Einstein, and a summarizing article in which he answers the criticism in the previous essays. This is most fascinating reading, but with all respect to the great physicist, I cannot accept his arguments against the philosophy of the quantum physicists. All essential points are treated in Bohr's article where he gives a delightful account of a number of discussions he had with Einstein. But the latter persists in his opposition, and declares himself firmly convinced that the present theory, though logically consistent, is an incomplete description of physical systems. His main arguments are not so much derived from considerations of causality, but from the new attitude to the meaning of physical reality which it implies. Let me quote his words (p. 672): "For me . . . the expectation that the adequate formulation of the universal laws involves the use of all conceptual elements which are necessary for a complete description, is more natural," namely than the ideas of the quantum physicists, and he insists that the emission of, say, an α-particle by a radioactive atom with definite energy must happen at a definite time predictable from theory—otherwise he calls the description conceptually incomplete. Yet he, himself, has taught us in the case of relativity that this argument is wrong. There you have an infinite number of equivalent inertial systems, each of which can be assumed to be at rest with the same right. But there is no way of deciding experimentally which is truly or absolutely at rest. Einstein's opponents pointed out that they regarded a description of the world as conceptually incomplete which denied the existence of a system absolutely at rest, even if there is no experimental way of finding it. This antirelativistic argument is just as strong as Einstein's antiquantistic, as everybody has experienced who was asked to conceive a light wave without a material ether as a carrier of the vibrations.

The generation to which Einstein, Bohr, and I belong was taught that there exists an objective physical world, which unfolds itself according to immutable laws independent of us; we are watching this process like the audience watches a play in a theatre. Einstein still believes that this should be the relation between the scientific observer and his subject. Quantum mechanics, however, interprets the experience gained in atomic physics in a different way. We may compare the observer of a physical phenomenon not with the audience of a theatrical performance, but with that of a football game where the act of watching, accompanied by applauding or hissing, has a marked influence on the speed and concentration of the players, and thus on what is watched. In fact, a better simile is life itself, where audience and actors are the same persons. It is the action of the experimentalist who designs the apparatus, which determines essential features of the observations. Hence there is no objectively existing situation, as was supposed to exist in classical physics. Not only Einstein, but also others who are not opposed to our interpretation of quantum mechanics, have said that under these circumstances there is no objectively existing external world, no sharp distinction between subject and object. There is of course some truth in it, but I do not consider this formulation to be very fortunate. For what do we mean by speaking of an objectively existing world? This is certainly a

prescientific notion, never questioned by ordinary man. If he sees a dog, he sees a dog whether it sits beside him, jumps about, or runs away and disappears in the distance as a tiny spot. All these innumerable and vastly different sense impressions are united by an unconscious process in his mind to the one conception dog, which remains the same dog under all these aspects. I propose to express this by saying that the mind constructs, by an unconscious process, invariants of perception, and that these are what ordinary man calls real things. And I think that science does exactly the same, only on a different level of perception, namely using all the magnifying devices which are the essence of observing and measuring.

The innumerable possible observations are linked again by some permanent features, invariants, which differ from those of ordinary perception, but are nevertheless in the same way indicators of things, objects, particles. For in describing what we observe even with the most refined instrument we have no other language that the ordinary one. Thus atomistic objects have, it is true, not all the properties of ordinary objects, but they have enough definite properties to ascribe to them physical reality of the same kind as to a dog. I think the fact that various observations of electrons give always the same charge, rest mass and spin, justifies perfectly speaking of them as real particles.

If we thus have to attribute a definite reality to the particles, what about the waves? Are they also real and in what sense? It has been said that electrons appear sometimes as waves, sometimes as particles, perhaps changing over every Sunday and Wednesday, as a great experimentalist mockingly remarked, obviously in a fit of anger about the somersaults of the theorists. I cannot agree to this view. In order to describe a physical situation, one has to use both waves, describing a "state," that is the whole experimental situation, and particles, the proper objects of atomic research. Though the wave functions are representing, by their square, probabilities, they have a character of reality. That probability has some kind of reality cannot be denied. How could, otherwise, a prediction based on probability calculus have any application to the real world? I am not deeply interested in the numerous attempts to make this more understandable. It seems to me, just as the necessity of the causal relations of classical physics, something beyond physics, a metaphysical idea. The same holds for the wave functions of quantum mechanics. One could call the use of particles and waves in physics a duality in the description, which should be strictly distinguished from complementarity.

Let us now finally ask whether these new developments in physics have any bearing on other subjects, and principally on the great problems of metaphysics. There is first the eternal dispute between idealism and realism in philosophy. I do not think that the new ways in physics can produce any weighty argument for one side or the other. Whoever believes that the only important reality is the realm of ideas, of the spirit, should not occupy himself with science. The scientist must be a realist, he must accept his sense impressions as more than hallucinations, as messages of a real outer world. In disentangling these messages he uses ideas of a very abstract kind, group theory in spaces of many or even infinitely many dimensions and things like that, but finally he has his observational invariants representing real things with which he learns to operate like any craftsman with his wood or metal. Modern theory has made the part of the ideas more extended and refined, but not changed the whole situation.

But a real enrichment of our thinking is the idea of complementarity. The fact that in an exact science like physics there are mutually exclusive and complementary situations which cannot be described by the same concepts, but need two kinds of expressions, must have an influence, and I think a welcome influence on other fields of human activity and thought. Here again Niels Bohr has shown the way. In biology the concept of life itself leads to a complementary alternative: the physicochemical analysis of a living organism is incompatible with its free functioning and leads in its extereme application to death. In philosophy there is a similar alternative in the central problem of free will. Any decision can be considered on the one side as a process in the conscious mind, on the other as a product of motives, implanted in the past or present from the outside world. If one sees in this an example of complementarity the eternal conflict between freedom and necessity appears to be based on an epistemological error. But I cannot enter into the discussion of these questions which are only just beginning to be seen in this way. Let me conclude by a remark on Russell's definition of metaphysics from which I started: that it is an attempt to conceive the world as a whole by means of thought. Has the lesson in epistemology which we learned from physics, any bearing on this problem? I think it has, in showing that even in restricted fields a description of the whole of a system in one picture is impossible; there are complementary images which do not apply simultaneously but are nevertheless not contradictory and exhaust the whole only together. This is, I think, a very healthy doctrine, which properly applied may remove many violent disputes not only in philosophy but in all ways of life.

Mathematicians at Ticonderoga

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Dr. Struik, a native of the Netherlands, is professor of mathematics at Massachusetts Institute of Technology. He received his education at the University of Leiden. He has written monographs and textbooks on different aspects of geometry and tensor calculus and is an editor of the Journal of Mathematics and Physics *as well as* Science and Society. *His contributions to the history of science are contained in his* Concise History of Mathematics *and in his* Yankee Science in the Making. *The latter is an account of the development of science in New England from its beginning to the Civil War. In trying to extend this study to the whole of North American science, especially in the colonial period, he came upon the material presented in this article, which is based on a paper he read at the organizing meeting of the Northeast Section of the American Mathematical Association at Durham, N.H., 26 Nov. 1955.*

THIS is a story about mathematically trained men who for a winter were all in or around Ticonderoga at the time of the French and Indian wars. The presence of at least four such men at one hotly contested spot is an indication of the interest shown by the Great Powers in engaging the services of scientifically competent men, notably in military engineering and in cartography. In telling this story, we are also contributing, in our own way, to the commemoration of the 200th anniversary of the old fortress, which is now a historic exhibit looking very much the way its architect, one of the heroes of this tale, conceived it.

At some time or another many of us have visited in our imagination the vast tracts of forest wilderness that stretched between the northern reaches of the Hudson and the Canadian frontier in the last days of New France when Albemarle and Amherst clashed with Montcalm and Lévis. We may have been reading Parkman, or Kenneth Roberts, or Cooper's *Last of the Mohicans*. There, along the shores of Lake George—Lac Saint Sacrement as the French called it—and Lake Champlain armies of British and French regulars, of colonial farmers and peasants and Indian auxiliaries moved north and south in guerilla warfare and regular battles, based on fortresses, of which Ticonderoga has remained the best known. A pleasant thing to contemplate, told by neither Parkman, nor Roberts nor Cooper, is that at the firesides of the contending parties we might have had a good chat on the best of 18th century mathematics.

After Braddock's defeat in the Monongahela wilderness in 1755, Parliament decided on the formation of a Royal American Regiment of Foot to be trained especially for forest fighting. A number of commissions went to foreign Protestants with military qualifications; a touch of Protestantism would be all to the good in a war with thoroughly Catholic New France. The Puritan fervor of the Yankee volunteers at Louisbourg in 1745 had been a case in point. One of the officers so enlisted was Joseph Frédéric Vallet Des Barres, born in 1721 or 1722 of French-Swiss stock, who had learned mathematics under the Bernoullis at the University of Basle and the military art at Woolwich Academy (*1*). He entered the Royal American Regiment as a lieutenant, and in 1757 we find him fighting Indians at Schenectady. Later in the year he was at Lake George, reconnoitering Ticonderoga, which was held by the French who had just built the fortress and baptized it Carillon. In the summer of 1758 he was engaged in engineering work at the siege of Louisbourg.

Another foreign-born Protestant in the American Regiment of Foot was Captain-Lieutenant Samuel Holland, born in 1728 in the Netherlands (*2, 3*). What he may have lost to Des Barres in formal education he had gained in military and surveying experience. An engineer and cartographer, he served in the Dutch campaign of 1747 and 1748, after which he went to England. In 1756 he was at Albany and at Fort Edward north of it with the rank of Lieutenant, and during 1757 as captain-lieutenant in the army around Ticonderoga, making charts of sections of New York Province. In 1758 we find Holland also as an engineer at Louisbourg. Many times after this Des Barres and Holland were thrown together in their careers as surveyors and cartographers.

The French-Canadian engineer in charge of construction at Carillon had also received a mathematical training. Michel Chartier de Lotbinière, born in Quebec, son-in-law of the royal engineer Chaussegros de Léry, friend and protégé of the royal governor the Marquis de Vaudreuil, also Canadian born, he was a member of New France's native semifeudal aristocracy (*4, 5*). He almost

certainly received his education at the Jesuit college in Quebec, where classical training was combined with the study of hydrography, which was a combination of mathematics, astronomy, navigation, and cartography; at any rate, he was in close touch with the professor of hydrography, Father De Bonnécamps (*6–8*). In 1736, at 13 years of age, he entered the army as a cadet, became a second ensign in 1744, and took part in the Acadian campaign of 1746 stationed at Grand-Pré, perhaps exchanging smiles with Longfellow's Evangeline. In 1749 the governor, La Gallissonnière, sent him out to the Great Lakes and the Mississippi on one of those military expeditions favored by this intelligent soldier-scientist (*9*); the order was given to keep eyes open for all objects of interest to the scientist and anthropologist and to report to the governor or, in his absence, to De Bonnécamps.

Lotbinière also collaborated with De Bonnécamps in determining the longitude of Quebec, which was probably done by observing the satellites of Jupiter. They settled on $4^h50'+$ west of Paris, which means $72°30'+$; since the true value is $73°33'$, the result was not particularly good, although it was better than the very first determination of this longitude by Deshayes in 1686, who had found $72°13'$ (*7, 10*). Thomas Brattle, as early as 1694, had found the longitude of Boston with an error of only about $20'$. The reason probably was the poverty of the instruments available at Quebec, of which Bonnécamps complained; all we know is that he got a 3-foot octant with telescopic sight. He was luckier with his latitude, which he set at about $46°48'$, which is also the present value. Lotbinière sent a paper on this work to the Paris academy, where Delisle read it February 1755.

From 1752 to 1754, Lotbinière was in France to study munition works and fortifications and to meet scientists under the egis of the now returned La Gallissonnière. When he was back in Quebec, Vaudreuil, who saw the storm coming, sent him to Lake Champlain to apply his knowledge of fortifications to build a new fortress in the best style of the age. Up to 1755 the southern entrance to Canada via Lake Champlain had been defended by Fort St. Frédéric, which had been fortified in 1743 by Chaussegros De Léry and built at present Crown Point (*11*). Vaudreuil decided to build his new fortress beyond Crown Point at the inlet from Lake Saint Sacrement. Here Lotbinière, from 1755 on, spent most of his time in constructing his fortress according to the best teachings of the school of Vauban. Short of Louisbourg, Carillon, by 1757, probably was the most modern fortress on the North American continent, and could hold its own in comparison with both Castle William in Boston Harbor and the old Spanish fortress at St. Augustine. Mathematicians should not underestimate the importance of the Vauban polygonal style: through the work of Monge, modern geometry has grown from it. Lotbinière also made astronomical observations, which he had continued in France, in order to determine the exact position of Carillon, but so far as we know none of this work has ever been published.

After the war with England had been officially declared, in 1756, France sent to Canada regular troops under the Marquis de Montcalm, who arrived the same year. With Montcalm as aide-de-camp was a dashing officer of 27 years, Louis Antoine de Bougainville. Born in 1729 as son of a Paris notary, Bougainville had moved in the circles of the academicians and the encyclopedists under the friendly eye of D'Alembert. In 1754 he had published a stately introduction into the integral calculus, to serve, as he said, as a sequel to L'Hospital's *Analyse des infiniment petits* of 1696, the first introduction to the differential calculus. By 1754 Bougainville had received a commission in the army, which did not prevent him from continuing his mathematical work. In 1756, when he was in London, the second volume of the integral calculus appeared, probably the first book exclusively devoted to differential equations (*12*). The proofs, in his absence from Paris, had been corrected by Bézout.

Both books were entirely up to date, embodying in a systematic way the research of the Bernoullis, Euler, D'Alembert, and others. They brought the author a membership in the Royal Society despite the unfavorable political atmosphere, and despite the fact that soon afterward the learned young officer sailed for Canada with Montcalm to fight the English. In Canada, Bougainville, brilliant representative of the Paris intellectual world, quick with the pen, ready with the épée, and favored by the ladies—even popular with the Indians, who seemed to have liked him—found with Montcalm his way into the gay provincial life of Quebec and Montreal—gay despite the ruin of the country—and then followed his general to Carillon, where he stayed during 1757 and 1758.

I would be enchanted to report the budding friendship of the two mathematically inclined young officers (as they walked arm in arm on the ramparts of Carillon, perhaps discussing Euler's or Tobias Mayer's contributions to the lunar theory, that application of the three-body problem which was so vitally important to cartographers and navigators. The sad story is that nothing of the kind happened—the two men had no use for each other. It may have been that the wretched monotony of

garrison life in a wilderness outpost grated on the nerves, especially of the Parisian gallant—a monotony which even the great victories of Montcalm at Fort William Henry and outside Carillon over Albemarle could hardly relieve; or it may have been the snobbish attitude of the European officer toward the colonial that was also endemic in the British army; or it may have been that Lotbinière was actually a little too close to some of the grafting Canadian officials for the taste of the French command—Bougainville had no patience with Lotbinière. "He wants to trace a meridian at Carillon," wrote Bougainville in 1758, "because this man, great engineer among the astronomers, is only an astronomer among the engineers. I have here a letter from M. DuHamel [secretary of the Académie] a botanist, great physicist and the ablest man in the Académie des Science for the culture of trees and lands, who writes me that M. DeLotbinière is an excellent engineer." He accused Lotbinière of incompetence and of graft (*13*). Montcalm shared his prejudice and in 1759 proposed for Lotbinière the canteen rather than the fortifications. It is not easy to sift truth from exaggeration, and we know little of Lotbinière's own reactions, although it is certain that corruption eroded the whole fabric of New France in its final years. It is also true that nerves were on edge by 1758–59, when the final collapse of Canada was near. At the same time, evidence seems to show that Lotbinière actually built the imposing fortress and that its fall without a fight in 1759 was as little his fault as its second fall without a fight in 1775 in the name of Jehovah and the Continental Congress. When, in 1759, the British took Carillon and renamed it Ticonderoga, their repair work followed Lotbinière's plans.

All four men, in their twenties or early thirties during their Ticonderoga days, lived to a ripe old age, one even reaching 103. With the exception of Lotbinière, all subsequently made distinguished scientific careers for themselves. Lotbinière, after the fall of his fortress and his country, lived the life of an exile in France and England, quibbling with other refugees, including Vaudreuil, brooding about the past, and trying to get his properties back. He succeeded in this, and Louis XVI made him a marquis, but he continued brooding. During the American Revolution he sided, like many Canadians, with the rebels and served in some mysterious capacity as an agent of Vergennes in America (*14*). At the time of his death in 1799, this quondam Scarlet Pimpernel on the side of rebellion resided in New York. Except Ticonderoga itself, now restored to full Lotbinièrian glory, he left no scientific work in his name, although some may still exist in manuscript.

Bougainville's later career is well known. He was with Montcalm on the Plains of Abraham, returned to France in 1760, served with distinction in the further pursuit of the Seven Years War, and from 1766 to 1769 made the famous voyage of discovery around the world for which he is mainly remembered. His *Voyage autour du monde* of 1771, and Diderot's subsequent *Supplément au voyage de M. de Bougainville* made his name familiar in his time, as do the showy climbers on Southern and Californian porches and pergolas in present days. We find him back on the American coast during the Revolution as a commander in the fleet of De Grasse, where he participated not only in action but also in testing some new instruments for measuring longitude, the chronometers (*15*). Later he continued to cultivate his old loves for the exact sciences, became member of the Institute and the Board of Longitudes, and died in 1814. He was to inspire Alexander Von Humboldt to go out on his famous voyage to America. I am not aware that he published anything more in the exact sciences—certainly not the third volume of his integral calculus that he had anticipated before he went to Ticonderoga.

Bougainville naturally brings to mind his British counterpart, Captain James Cook, who went out on that equally famous tour around the world in 1776–79 from which he never returned. It so happens that Cook also enters into our story. When Des Barres and Holland were at Louisbourg in 1758, they met Captain Simcoe, father of the late governor of Canada. Let Holland tell the story himself (*2*, pp. 18–19):

"The day after the surrender of Louisbourg, being at Kensington Cove surveying and making a plan of the place, with its attack and encampments, I observed Captain Cook (then master of Capt. Simcoe's ship, the *Pembroke* man-of-war) particularly attentive to my operations, and as he expressed an ardent desire to be instructed in the use of the Plane Table (the instrument I was then using), I appointed the next day in order to make him acquainted with the whole process . . ."

The lessons continued in Halifax. The great cabin of the *Pembroke* became dedicated to scientific purposes. Under Captain Simcoe's eye, Holland and Cook compiled material for a chart of the Gulf and River of St. Lawrence, and another one of the Gaspé approaches, to be used during the campaign for the conquest of Quebec, which was then being prepared under Wolfe (*16*). Existing maps, especially those so far possessed by the British, were highly unsatisfactory, while the French had done some excellent work, especially through the efforts of Chabert, who had been taking meas-

urements of the Nova Scotia coast in 1750–51 with headquarters at Louisbourg. Cook thus got his start in scientific navigation under the eye of Holland, also assisted by Des Barres. It served him well on his great voyages, where he sailed beyond the Bering Strait. He also tested chronometers, in this case the Harrison type, whereas Bougainville, about the same time, tested the French type of Le Roy and Berthoud.

Both Des Barres and Holland spent much of their time during the next decades in mapping the coast and coastal districts of the North Atlantic Coast from Labrador to Massachusetts, Des Barres in the service of the Admiralty, Holland as surveyor general under the Commissioners for Trade and Plantations. The British authorities, seeing trouble in the making, were anxious to have the best possible charts of the American coasts. When the trouble actually arrived, both Des Barres and Holland stayed faithful to the Crown and continued their work in Canadian regions. The fruits of much of their labor were laid down in that famous cartographic work, the *Atlantic Neptune,* on which Des Barres spent years of his life and a good part of his fortune (*17*). It carries his name, even though Holland also contributed to it. Beginning about 1777, the maps were published both in atlases and separately, and since Des Barres, who had the engravings, was always correcting them, the number of stray charts is large, and they are a source of delight and despair to all ambitious map collectors. It was in its day an impressive work, ". . . one of the most remarkable products of human industry that has even been given to the world through the arts of printing and engraving . . ." as was said in a Paris journal of 1784. Des Barres later became governor of Cape Breton, where he is remembered as the founder of Sydney; later he became governor of Prince Edward Island. He died in Halifax in 1824, at 103 years of age. Holland had preceded him in 1801, at Quebec.

The two books of Bougainville on the integral calculus are the work of a talented pupil of D'Alembert, eager in studying the books and papers on the subject including the most recent of Euler, Maclaurin, and his teacher, capable of presenting the scattered material in the ordered form of a textbook, but not pretending to any originality except that of exposition. He is perfectly honest about it, and in the introduction to the first volume, after he has given a summary of the literature he has used and expressed his indebtedness to D'Alembert, he ends by "warning that there is nothing new by me in this work except the order that I have tried to put into the different methods, and form which I give to them, and which may perhaps serve as to make them understood." We must not forget, however, that writing a textbook of integral calculus about 1750, and an up-to-date one at that, was still a work of exceptional merit; there certainly was not much by way of precedent.

Bougainville himself wanted his treatise to be considered as the sequel to L'Hospital's *Analyse des infiniment petits,* the very first introduction in book form of the differential calculus as Leibniz and the Bernoulli brothers had developed it since 1684, and which had appeared in 1696. That a book could appear in 1752 and be called a sequel to it shows how little expository work had been accomplished in half a century of brilliant research. And indeed, Bougainville's book reminds us of L'Hospital's, not only in the use of the symbolism, but in its character as well; L'Hospital was also a talented pupil, in his case of the Bernoullis, who was capable of presenting their work and that of Leibniz in the ordered form of textbook, but not pretending to any originality. L'Hospital's book had received a translation into English by Stone that was published in 1730; Stone had added an integral calculus, but all in fluxion language (even L'Hospital's text itself) and giving not much more than Newton himself had published in his *Quadratura curvarum* of 1704.

Bougainville only had one competitor, and that was a woman. Maria Gaetana Agnesi of Milan had published her *Instituzione analitiche* in 1748, a magnificent two-volume work with not only the formal differential and integral calculus of her days, but also a wealth of applications to plane curves—we all have heard of the "witch" of Agnesi. Bougainville pays tribute to the lady: "Ainsi l'Italie qui a été la berceau de l'Algèbre, a produit aussi l'ouvrage le plus étendu que nous ayons sur la nouvelle Analyse. L'illustre Académicienne dans la partie de son livre destinée au Calcul intégral suit un ordre qui repand un grand jour sur cette matière"—however, her work needs completion and that is what Bougainville proposes to do.

Another book which influenced Bougainville, apart from Newton's *Theory of Fluxions,* which had only appeared in 1736, and Maclaurin's book of the same name, of 1742, and which presented the most advanced English work, was Euler's *Introductio in analysin infinitorum,* of 1748. We see it in his use of the formula

$$\sin z = \frac{e^{iz} - e^{-iz}}{2i}$$

in this form, except that $\sqrt{-1}$ is writen for i; which shows how close the notation already is to ours. However, this book by Euler was not on the calculus. When Euler began to write his differential

calculus (published in 1755) and his integral calculus (1768–70), both L'Hospital and Bougainville soon became antiquated to such an extent that Bougainville's books are hardly or not at all mentioned in the histories of mathematics. Even Cantor has little to say.

Yet, the books are not without interest as a summary of what was known about 1750 in the most sophisticated Paris circles interested in mathematics. The first volume is a treatise on indefinite integrals, beginning with that of $ax^n\,dx$ and ending with elliptic integrals, still in the early form in which Maclaurin and D'Alembert had studied them. Particular attention is paid to dx/x because this integral was not yet understood by L'Hospital—that is, by the Bernoullis of 1697—though Leibniz in his very first paper, of 1684, had already recognized its logarithmic character. There is thus an introductory chapter on logarithms and another on inverse trigonometric functions. It is recognized that they are related through the imaginary—hence another chapter on imaginaries culminating in what amounts to D'Alembert's demonstration that each polynomial of degree n in x with real coefficients can be split into linear and quadratic factors with real coefficients. This again is useful for the integration of rational fractions.

The second volume is devoted to ordinary differential equations and may, as said before, very well be the first textbook written exclusively on this subject. We find already some familiar terms: *exact equations, homogeneous equations, equations of the Riccati type*. There are also simultaneous equations and equations of higher order with constant coefficients and some others that can be derived from them. All in all, it is a creditable performance, and the author would have been competent to write that third volume on applications to geometry, astronomy, mechanics, and physics which he had in mind when he wrote the first of his books, if the Seven Years War had not detracted him from the pursuit of the mathematical sciences and led him into military and geographical adventures. His glory in the field of exploration of the earth has obscured his merits as a mathematician.

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13. R. de Kerallain, *Les Français au Canada*. La jeunesse de Bougainville et la guerre de sept ans (Paris, 1896). On the lack of harmony at Carillon see also M. Leland (*3*, pp. 43–45).
14. "Passé en France en 1761, revenu au Canada et retourné en France, ce personnage assez mystérieux semble avoir exercé des missions d'information pour le compte du gouvernement de Versailles" (from a letter by the director of the Archives de France, Paris, 7 Apr. 1955). M. Leland writes me on that account: "If one could actually find the Vergennes instructions to which Lotbinière refers, it would help. Until they are discovered I'll stick to my belief that they are a figment of the marquis' own deteriorating mind," and quotes as an example some letters printed in the fifth series, vol. 3 of the American Archives (letter of 26 Apr. 1955).
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Population Movements in the Southern United States

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URBANIZATION has been the dominant demographic process in the South for many decades (*1*). The shift of population toward urban residence in this most rural of the nation's regions has been in evidence since the beginning of the 19th century (*2*). Following generally the process in the nation, the rate of urbanization in the South has nevertheless until recently continuously lagged behind. However, the rapid acceleration of urban growth in the region during the first half of this century, notably since the first World War, has substantially narrowed the gap between the extent of urbanization in the region and that in the rest of the United States (*3*). Indeed, the momentum of urbanization in the South in recent years has attracted widespread interest and attention among scholars and laymen alike.

The process of urbanization has meant, among other things, a redistribution of the region's population. Involved in this redistribution has been an increase in the number of urban centers and a growth in the size of these centers. Since modern cities typically are not peopled primarily through the reproductive efforts of their own residents, these urban centers of the South have been dependent for most of their growth on a continuous influx of migrants. The eddies and currents of these urbanward migrant streams comprise the subject matter of this article. An attempt will be made to add some details regarding such aspects of the recent population movements as the sources and destinations of migrants, the approximate magnitude of the population transfer, and some conception of the population redistribution that has taken place (*4*).

The net migrants to urban centers of the South have originated, for the most part, in the rural areas of the region. Southern cities have been ideally situated to receive rural migrants (*5*). They are scattered about a region in which resides the most prolific large group of people in the nation —the southern farm population. So prodigious has been their yield of progeny that these people are often said to constitute the "seedbed" of the nation. While rural southerners have generally been characterized by high birth rates, those living in the more remote or inaccessible sections have reproduced most rapidly. Natural increase in the rural areas has far outrun the absorptive capacity of southern agriculture. Less than half the youth reaching maturity on the region's farms have found it either possible or desirable to obtain footholds in agriculture. At the same time, urban areas, with lower birth rates and fast developing industry, offered economic opportunity. Large-scale migration to cities has been inevitable. Although the region's urban centers have been the destination for a majority of the migrants, many of them have found their way to cities outside the South.

What has been the magnitude of this movement from southern farms? The best answer available is provided by the Department of Agriculture's annual estimates of migration to and from farms of the Census South (*6*) between the years of 1920 and 1954, inclusive (Fig. 1). These estimates reveal that southern farms have exported annually, with few exceptions, a large human cargo to urban areas. The cumulative net gain to nonfarm territory during the 35-year period was 14,555,000. This balance resulted from the departure of 31,243,000 migrants from the region's farms and the arrival of 16,688,000 on these farms. Thus, according to these Government estimates, almost 48 million persons moved to and from farms in the South, in order to produce the net gain of 14,555,000 for nonfarm territory.

Only two brief periods in the 35-year interval witnessed the reversal of the net movement to urban areas from southern farms. The first, a de-

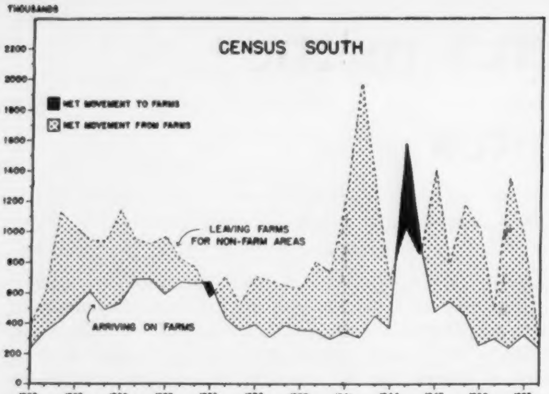

Fig. 1. Movement of persons to and from farms in the Census South, 1920–1954 (*12*).

pression phenomenon, was in 1932, when youth who ordinarily would have migrated under more normal economic conditions were dammed up on the farms of the region. The second interruption of the net urbanward movement was in 1945–46, when the reconversion of industry from all-out war production and the demobilization of the armed forces sent thousands back to the land. Otherwise, farms in the region sustained net losses of migrants every year.

The magnitude of the estimated annual migration gains by urban areas varied widely, occasionally even in successive years. The largest gain occurred in 1942, when 1,670,000 more persons left southern farms than arrived on them. The military services and booming industry combined forces to drain this all-time record number of persons from the region's farms. The second largest gain, 1,119,-000, was registered in 1952. At the other extreme, the lowest net gain by urban areas, 99,000, was registered in 1954, the last year for which data are available. The next smallest gain, 108,000, occurred in 1931 when the spreading effects of the depression, which in 1932 brought a reversal of the net movement, were sharply curtailing migration from farms. In most of the years, however, the annual gains made by urban areas at the expense of the farms of the South ranged between 200,000 and 500,000.

Similar data for other regions reveal that farms in the South have contributed more migrants to towns and cities during the last three decades than the farms in the rest of the nation combined. The bulk of the migrants leaving southern farms undoubtedly reached points of destination in the region. That substantial numbers, however, moved to urban areas in the North and West is demonstrated by the state-of-birth data of the U.S. Census for 1930 and 1940, and implied by those for 1950. These materials show that the South in recent decades has consistently sustained net losses as a result of interregional shifts of population. In 1950, for example, almost 4 million more persons born in the Census South were living elsewhere than were born elsewhere and living in the Census South. Although southern cities have drawn considerable numbers of migrants from other regions, the urban centers outside the South have made much heavier gains through migration from the South.

Cityward migrants in the South have not originated only in rural areas. Heavy contingents of migrants have moved from one urban center to another within the region. While those from the rural areas have comprised the net migrants to urban territory, the movers transferring between cities have brought about profound changes in the distribution of the region's urban population. Something of the magnitude of this interurban movement is suggested by the internal migration data collected by the 1940 census for the 1935–40 period and by the 1950 census for the 1949–50 period.

In this connection, let us consider briefly the 1940 internal migration data. For this purpose migrants were defined as "those persons who lived in 1935 in a county (or quasi-county) different from the one in which they were living in 1940." According to this definition, 15.1 percent of the urban population and 8.8 percent of the farm population of the Census South were classified as migrants. The corresponding percentages for the nation as a whole were 11.1 and 10.1. Thus, migrants in the South as compared with those in the nation were proportionately somewhat more numerous in the urban centers and less numerous in farm areas. Within the region migrants were almost twice as numerous among urban as among farm people.

The cross tabulation of type of residence in 1935 with type of residence in 1940 sheds considerable light on the volume of movement among urban centers notwithstanding the acknowledged overstatement of urban residence in 1935. These data show that, of the migrants in the urban South in 1940, three out of five (60.5 percent) reported residence in other urban territory in 1935, approximately one out of five (21.4 percent) on farms, and almost one out of five (18.9 percent) in rural-nonfarm territory. The remaining 3.2 percent reported rural residence without specifying farm or nonfarm residence. Allowing for substantial overstatement of urban residence in 1935, the fact remains that large numbers of migrants moved between cities of the region between 1935 and 1940. This transfer was undoubtedly more frequent than the more highly publicized rural-to-urban migration. Moreover, this same pattern has probably ex-

Table 1. Relative and absolute population change in the 13 southern states, by race and residence, 1930–1940 and 1940–1950 (*10*).

South	1930	1940	Absolute change	Percentage change	1940	1950	Absolute change	Percentage change
Total	33,771,653	37,013,087	3,241,434	9.6	37,013,087	41,728,272	4,715,185	12.7
Urban	10,827,860	12,873,317	2,045,457	18.9	12,873,317	17,912,757	5,039,440	39.1
Rural-farm	15,541,106	15,523,729	−17,377	−0.1	15,523,729	11,275,471	−4,248,258	−27.4
Rural-nonfarm	7,402,687	8,616,041	1,213,354	16.4	8,616,041	12,540,044	3,924,003	45.5
White	24,843,853	27,651,141	2,807,288	11.3	27,651,141	32,212,529	4,561,388	16.5
Urban	8,180,326	9,671,227	1,490,901	18.2	9,671,227	13,872,675	4,201,448	43.4
Rural-farm	10,911,555	11,091,702	180,147	1.7	11,091,702	8,160,530	−2,931,172	−26.4
Rural-nonfarm	5,751,972	6,888,212	1,136,240	19.8	6,888,212	10,179,324	3,291,112	47.8
Nonwhite	8,927,800	9,361,946	434,146	4.9	9,361,946	9,515,743	153,797	1.6
Urban	2,647,534	3,202,090	554,556	20.9	3,202,090	4,040,082	837,992	26.2
Rural-farm	4,629,551	4,432,027	−197,524	−4.3	4,432,027	3,114,941	−1,317,086	−29.7
Rural-nonfarm	1,650,715	1,727,829	77,114	4.7	1,727,829	2,360,720	632,891	36.6

isted for the last quarter of a century. Unquestionably this interurban movement has had a large part in redistributing the urban population of the South.

The variety of migrations involving exchanges of population among urban, rural-nonfarm, and rural-farm areas of the South during the past two decades is reflected in the population changes that occurred in these three residential categories from 1930 to 1940 and 1940 to 1950. Indeed, in view of the fragmentary nature of our data on the migrations themselves, the resulting population changes represent one of our best clues about the nature of these population movements.

Between 1930 and 1940 the total population of the 13 southern states registered a gain of 9.6 percent (Table 1). However, the urban population increased at the rate of 18.9 percent and the rural-nonfarm at the rate of 16.4 percent, while the dwellers on farms, despite their high fertility, decreased slightly (−0.1 percent).

When these population changes during the 1930's are analyzed by race, sharp differences between the whites and the nonwhites emerge (*7*). White southerners gained at the rate of 11.3 percent as contrasted to a gain of 4.9 percent by nonwhites. Residential breakdowns within each race further indicate that white urban and rural-nonfarm dwellers increased rapidly (18.2 and 19.8 percent) and that white farm people registered the small but appreciable gain of 1.7 percent. Nonwhites were characterized by a contrasting pattern of residential changes. Those in urban centers increased rapidly, by 20.9 percent, but nonwhites in rural-nonfarm areas realized only a 4.7 percent increase and those on farms declined substantially, with a loss of 4.3 percent. Nonwhites, then, while failing even to hold their own on farms, were urbanizing fast. Moreover, as compared with whites, relatively little of their urbanization consisted of movement to suburban areas. Nonwhites tended, rather, to move directly to urban centers of the region or leave it entirely.

The 1940–50 decade brought an acceleration of the changes that had occurred in the previous decade. The total population of the South gained by 12.7 percent. Residents of urban centers and rural-nonfarm areas registered the tremendous gains of 39.1 and 45.5 percent, respectively, but farm dwellers declined sharply in number by more than 4 million, or 27.4 percent (*8*).

A comparison of the total population of the two races reveals that whites experienced the heavy gain of 16.5 percent, while nonwhites, by registering a relative gain of only 1.6 percent, did only slightly better than hold their own. From a residential standpoint, whites gained heavily in both urban and rural-nonfarm areas, specifically by 43.4 and 47.8 percent, respectively, but registered the noteworthy loss of 26.4 percent in their farm population. Nonwhites continued to gain in urban centers (26.2 percent), though much less rapidly than whites, but also realized a heavy gain in rural-nonfarm areas (36.6 percent). The relative loss of farm population was even more drastic among nonwhites (29.6 percent) than among whites.

These data vividly show the progressive depopulation of farm areas in the South, coupled with the rapid growth of urban centers. Further insight may be gained from the consideration of population change during the last two decades in the counties of the South classified according to size of the largest center of population in each in 1950 (Fig. 2). Between 1930 and 1940, the largest relative population gains (19.3 per cent) were registered in combined counties containing cities with populations of 100,000 or more. The next largest rate

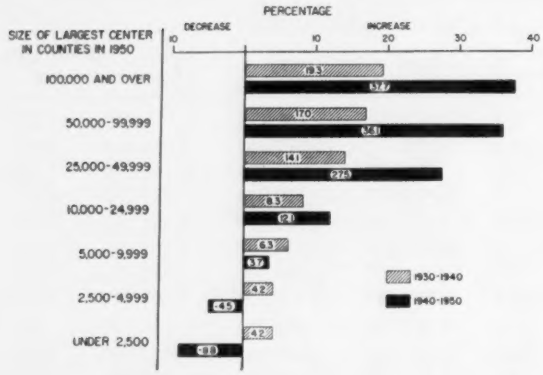

Fig. 2. Population changes in the counties comprising the 13 southern states, grouped according to largest center in each county in 1950, 1930–40, and 1940–50 (*13*).

of growth, 17.0 percent, prevailed in the group of counties whose largest center ranged from 50,000 to 99,999 in size. Each step downward in size of largest center was accompanied by a lower rate of growth. The combined counties with cities of from 25,000 to 49,999 increased by 14.1 percent. Those containing centers of from 10,000 to 24,999 population increased by 8.3 percent; those with centers from 5000 to 9999 registered a gain of 6.3 percent; and the group of counties having cities between 2500 and 4999, as well as those with centers with fewer than 2500 inhabitants, gained only 4.2 percent. In general, the larger the center in the county, the greater was the relative population growth between 1930 and 1940.

During the decade of the 1940's the same pattern prevailed, although gains were even greater in the combined counties that contained the larger cities; and at the other end of the scale, the group of counties with smaller centers actually sustained losses of population. The aggregate population of counties containing centers of 100,000 or more increased by 37.7 percent; those whose largest centers were from 50,000 to 99,999 increased by 36.1 percent; those with centers of from 25,000 to 49,999 by 27.5 percent; those with cities of from 10,000 to 24,999 gained by 12.1 percent; and counties with centers of from 5000 to 9999 as a group registered a population gain of 3.7 percent. On the other hand, the counties whose largest centers were from 2500 to 4999 declined in population by 4.5 percent, and the remaining category of counties, with centers of under 2500 persons, sustained a population loss of 8.8 percent. The pattern is clear and consistent. The larger the population center (above 5000 persons) in a southern county, the greater was the population gain between 1940 and 1950. The aggregate of counties with centers of under 5000 persons lost; and the smaller the center, the greater the loss. Obviously there has been a tremendous movement of population toward the larger cities of the South. Indeed, the gain of population in the smaller cities generally has not been sufficient to offset the loss of population in the surrounding rural territory, resulting in a decrease of population for the counties in which they are located.

In the South, as elsewhere in the nation, the destination of migrants has not been solely the cities, but also suburban areas adjacent to but outside of urban centers. This phenomenon of suburbanization in the South is dramatically illustrated by the contrasting rates of population change in the following component parts of counties containing centers of 100,000 persons or more in 1950: central cities, farm areas, other (or suburban) areas (see Table 2). In the counties containing these large centers in 1950, between 1930 and 1940, the combined central cities increased in population by 13.7 percent, the combined farm areas by 11.5 percent, but the other (or largely suburban) areas of these counties registered a spectacular gain of 49.8 percent. Between 1940 and 1950, the central cities increased in population by 32.0 percent, the farm areas declined by 37.8 percent, and the suburban areas registered the "whopping" gain of 85.5 per cent. In both decades the suburban areas experienced the largest increases, reflecting the heavy settlement of migrants outside of, but in close proximity to, these large urban centers. Other data show that the same movement to the fringe areas

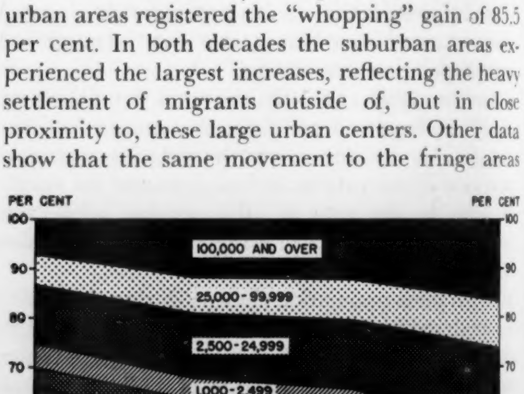

Fig. 3. Changing distribution of the population of the 13 southern states among specified classes, 1920–50 (*14*).

Table 2. Population changes in the farm, central city, and other (suburban) areas of the counties of the 13 southern states with cities of 100,000 or more in 1950, 1930–40 and 1940–50 (*11*).

Residential category	1930	1940	Absolute change	Percentage change	1940	1950	Absolute change	Percentage change
Total	5,930,824	7,073,924	1,143,100	19.3	7,073,924	9,741,216	2,667,292	37.7
Farm	455,777	508,170	52,393	11.5	508,170	316,187	– 191,983	– 37.8
Central cities	4,531,934	5,153,362	621,428	13.7	5,153,362	6,804,921	1,651,559	32.0
Other (suburban)	943,113	1,412,392	469,279	49.8	1,412,392	2,620,108	1,207,716	85.5

around smaller cities in the region has taken place and is continuing at a rapid pace.

Finally, it is appropriate to consider the extent of urbanization that has been realized, largely as a result of population movements, in the South. The net effect of the shifting and shuffling of southerners through territorial moves in the past quarter of a century has been to increase dramatically the proportion of the people residing in urban centers and to decrease the proportion living on farms. To cite specific figures, the percentages of the population of the 13 southern states in urban areas in 1930, 1940, and 1950, were 32.1, 34.8, and 42.9, respectively (*9*). The corresponding percentages on farms were 46.0, 41.9, and 27.8. The proportion of population in rural-nonfarm areas increased slightly in the 1930's, from 21.9 to 23.3 percent, and much more rapidly in the 1940's, reaching 30.1 percent in 1950. Notwithstanding the label "rural-nonfarm," a large share of the people so classified, certainly more than half of them, are in no sense rural. The increase of this category is part and parcel of the urbanization process. This process, involving large-scale migration to southern cities, has brought the region's urban population to an impressively high relative level while reducing the number of farm dwellers to only 27 out of every 100 persons.

Additional understanding of the redistribution of the South's population that has resulted from the multitude of currents and cross-currents of migration is provided by the changing distribution of the region's population among centers of different sizes (Fig. 3). Each recent decade has witnessed an increase in the proportion of the population residing in cities of 100,000 or more. Proportionately, more than twice as many were so classified in 1950 as in 1920, 16.6 as compared with 7.2 percent. The proportion of the South's people living in centers of from 25,000 to 99,999 increased from 6.3 percent in 1920 to 9.6 in 1950. The corresponding increase in centers of from 2500 to 24,999 was from 11.9 to 16.7. The proportion living in centers ranging in size between 1000 and 2499 actually declined from 4.7 percent in 1920 to 4.0 percent in 1950. The percentage residing in centers of under 1000 persons decreased sharply from 4.1 in 1920 to 2.7 in 1950. Interestingly enough, the proportion of the population outside of all centers and not on farms increased from 11.7 in 1920 to 23.4 percent in 1950. This latter increase is due largely to the heavy movement to suburban and fringe territory beyond the corporate limits of cities that has already been mentioned.

In this analysis, several types of data, each relating to a different aspect of migration in the South, have been cited. When pieced together, these data provide a composite picture of one of the nation's most important demographic and economic developments of this century—the urbanization of the South. This process, although it is dependent to a much greater extent on human resources of the region, has been generally similar to that which occurred earlier in the Northeast and Far West. Rural areas of the South have furnished the migrants (substantial numbers of whom have also reached cities outside the region) and have been progressively depopulated while doing so. Urban centers and urban fringe areas have grown rapidly, and the people of the South are increasingly becoming concentrated in the larger cities and metropolitan areas. Moreover, interurban migrations are even more frequent than the more highly publicized rural-urban movements. The combined waves of urbanward migration have swept the South into a new era of urbanization, metropolitan growth, and economic development. So dramatic has been the transformation that the degree of change taking place and the implications of this change are only beginning to be recognized.

References and Notes

1. The most comprehensive and up-to-date treatment of the urbanization of the South and the implications of this urbanization for the region and for the nation is *The Urban South*, R. B. Vance and N. J. Demerath, Eds. (Univ. of North Carolina Press, Chapel Hill, 1954).
2. In this analysis, the terms *southern United States* and *the South* are used synonymously and, unless otherwise specified, this region is considered to include the following 13 states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi,

North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.

3. The rise of southern cities and the process of urbanization in the South as compared with those in other regions have been treated by T. L. Smith, "The emergence of cities," in *The Urban South,* R. B. Vance and N. J. Demerath, Eds. (Univ. of North Carolina Press, Chapel Hill, 1954), pp. 24–37.
4. For an analysis of the selectivity of the migration to southern cities, see H. L. Hitt, "Peopling the city: migration," *ibid.* pp. 64–77.
5. H. L. Hitt, "Migration and southern cities," in *The Sociology of Urban Life,* T. L. Smith and C. A. McMahan, Eds., (Dryden, New York, 1951), pp. 319–334.
6. The Census South includes, in addition to the 13 states of the region as delineated for this analysis (2) and frequently referred to throughout this article, Delaware, Maryland, West Virginia, and the District of Columbia.
7. For all practical purposes, the terms *nonwhite* and *Negro* may be considered synonymous. Approximately 99 percent of the nonwhite population of the South is Negro, the remaining 1 percent being divided among Indians, Japanese, Chinese, and other races.
8. Farm population data for 1950 and previous censuses are not strictly comparable because of the change in definition in 1950 that had the effect of excluding some persons who would previously have been included in this category. A portion of the absolute and relative decline (estimated to be about 9 percent) of farm population between 1940 and 1950 would have resulted from the change in the definition alone.
9. According to the new definition of the word *urban* that was introduced in the 1950 census (which is not comparable to the definition that had been used in previous censuses), the percentage of the population of the 13 southern states residing in urban areas in 1950 was considerably higher (47.1).
10. Basic data compiled from U.S. Bureau of the Census, *U.S. Census of Population: 1950,* vol. 2, *Characteristics of the Population* (Government Printing Office, Washington, 1952), Table 13 of pt. 2, p. 30; pt. 4, p. 30; pt. 10, p. 31; pt. 11, p. 39; pt. 17, p. 26; pt. 18, p. 26; pt. 24, p. 22; pt. 33, p. 37; pt. 36, p. 30; pt. 40, p. 26; pt. 42, p. 34; pt. 43, p. 63; pt. 46, p. 31.
11. Basic data compiled from U.S. Bureau of the Census, *U.S. Census of Population: 1950,* vol. 2, *Characteristics of the Population* (Government Printing Office, Washington, 1952), Tables 6 and 49 of pts. 2, 4, 10, 11, 17, 18, 24, 33, 36, 40, 42, 43, and 46; *Sixteenth Census of the United States: 1940, Population,* vol. 2, *Characteristics of the Population* (Government Printing Office, Washington, 1943), Table 27 of pts. 1–7; *Fifteenth Census of the United States: 1930,* vol. 3, *Composition and Characteristics for Counties, Cities, and Townships* (Government Printing Office, Washington, 1932), Table 14 of pts. 1 and 2.
12. Source: Agricultural Marketing Service, U.S. Department of Agriculture, *Farm Population—Migration to and from Farms, 1920–54* (Washington, D.C., 1954), p. 13; Agricultural Marketing Service, U.S. Department of Agriculture, *Farm Population—Estimates for 1955* (Washington, D.C., 1955), p. 4.
13. Basic data compiled from U.S. Bureau of the Census, *U. S. Census of Population: 1950,* vol. 2, *Characteristics of the Population* (Government Printing Office, Washington, 1952), Table 6 of pt. 2, pp. 12–17; pt. 4, pp. 10–18; pt. 10, pp. 10–13; pt. 11, pp. 11–21; pt. 17, pp. 9–14; pt. 18, pp. 9–12; pt. 24, pp. 9–12; pt. 33, pp. 12–18; pt. 36, pp. 12–16; pt. 40, pp. 10–14; pt. 42, pp. 11–18; pt. 43, pp. 17–28; pt. 46, pp. 12–15.
14. Basic data compiled from U.S. Bureau of the Census, *U.S. Census of Population: 1950,* vol. 2, *Characteristics of the Population* (Government Printing Office, Washington, 1952), Tables 3 and 13 of pt. 2, p. 7, p. 30; pt. 4, p. 7, p. 30; pt. 10, p. 7, p. 31; pt. 11, p. 7, p. 39; pt. 17, p. 5, p. 26; pt. 18, p. 6, p. 26; pt. 24, p. 6, p. 22; pt. 33, p. 8, p. 37; pt. 36, p. 7, p. 30; pt. 40, p. 7, p. 26; pt. 42, p. 8, p. 34; pt. 43, p. 10, p. 63; pt. 46, p. 8, p. 31.

"It is instructive," said Sir Oliver Lodge, "to realize the state of mind which misses a discovery as well as, what is more commonly attended to, the more admirable state of mind which succeeds." Many experimenters had opportunities as good as Röntgen's to observe the x-rays which were generated in their laboratories. Sir Oliver Lodge cited the case of Rev. Frederick Smith who, on finding that the plates wrapped in a box near a tube were fogged, was—so to speak—annoyed at this disturbance of his experiments, and kept the plates out of the way.—J. C. CHASTON, *Nature* 151, 55 (1943).

The Sun's Energy

FARRINGTON DANIELS

Dr. Daniels is chairman of the department of chemistry at the University of Wisconsin. After receiving his degrees at the University of Minnesota and Harvard University, he taught at Worcester Polytechnic Institute and served for a time as electrochemist at the fixed nitrogen laboratory of the U.S. Department of Agriculture. He has been connected with the University of Wisconsin since 1920. Between 1944 and 1948 Dr. Daniels was active in the work of the Manhattan District Project and the Argonne National Laboratory. His chief research interests are chemical kinetics, photochemistry, photosynthesis, atomic energy, crystals, nitrogen fixation, and utilization of solar energy. This article is based on a paper that was presented at the World Symposium on Applied Solar Energy at Phoenix and Tucson, Arizona, 31 Oct.–5 Nov. 1955, under the sponsorship of the Association for Applied Solar Energy, the Stanford Research Institute, and the University of Arizona.

LIFE without the sun is unthinkable. Modern civilization with its ever-increasing demands for energy is completely dependent on the solar energy of the past, in the form of fossil fuels as well as on the solar energy of the present. In this article, I am going to suggest how, by means of science and technology, we can obtain still more useful energy from solar energy. We rebel against the old adage that "there is nothing new under the sun."

Awakened Interest in Solar Energy

But why this *new* and almost explosive interest in utilizing solar energy? The sun has always been with us. I am delighted to see this new appreciation of solar energy—but I am worried also. For 7 years I have been actively urging scientists and engineers throughout the world to turn more of their research activities toward greater, direct utilization of this enormous, neglected source of energy—and now the idea has caught on like a delayed-action fuse. Too many people are beginning to expect too much too soon. There is no sudden era of solar prosperity just around the corner. There is much to be done yet by scientists, inventors and engineers, and philanthropists, before it is time for investors to become excited. The producers of conventional power through coal, petroleum, water power, and electricity have nothing to worry about. None of our present engines and generators will be rendered obsolete by solar energy. Atomic energy and solar energy each in its own way will merely supplement the *new* additions to our power-producing machinery. Atomic energy will come in large, multimillion-dollar central power stations near cities and towns. In fact, it is already here. The critical mass of uranium, the elaborate controls and shielding, and the disposal of radioactive waste, all work against small atomic units. Emphasis is being placed on 100,000-kilowatt atomic power units and larger, with some consideration of 5000-kilowatt units for special purposes. Undoubtedly, the successes of atomic power have had an influence on solar energy. They have weaned us away from thinking only in terms of conventional combustion fuels.

Unlike atomic energy, solar energy has no critical mass, no health hazards (except sunburn), and no waste products to dispose of. Anyone can go out in his yard and run a toy steam engine with free sunshine. There is no lower limit. Laboratory research and even pilot-plant operation for solar energy are comparatively inexpensive.

Solar-energy utilization will probably start with small units costing not millions but only thousands of dollars. They will find their first practical uses in rural, nonindustrialized areas. I do not mean to imply that the deserts of Arizona may not bloom with huge solar power plants—but they will not be important in 1956.

Let me ask again why do we sense this upsurge of interest in the utilization of solar energy now, and why has the direct use of the sun's energy been neglected so long? There are many reasons. Life has been too easy with concentrated energy in the form of coal, petroleum, natural gas, and water power. "Necessity is the mother of invention," and the scientists and engineers most capable of developing devices for making direct use of the sun have lived in industrialized countries where there has been no necessity for developing solar energy. These countries would not be industrialized unless they had plenty of fuel. Economically competition with inexpensive fossil fuels has been unattractive, and politically and socially there *has* been no demand in the industrialized countries that could not be met with conventional sources of energy.

But these conditions are changing, and there are

several factors that are contributing to our new interest in solar energy. We realize, as never before, that our fossil fuels—coal, oil, and gas—will not last forever. Several careful studies have been published in the last few years that point out that the depletion of our reserves will come sooner than we think. In the fuel-rich United States, the problem may be one for our grandchildren, but some countries already are feeling the pinch of a decrease in high-grade, easily minable coal. Moreover, the population of the world is increasing rapidly, and the demands for abundant energy are increasing still faster. Any estimates of the life of fuel reserves based on consumption at past rates are utterly unrealistic.

An occasional catastrophe has shown the helplessness of industrialized cities when the electricity goes off. Every year, it becomes more difficult for cities to exist without abundant power. Nor is this only an urban problem. The farms are mechanized, too. I have recently had a survey conducted by V. Stoikov, who found that for every calory obtained from food in the United States, we put a little more than a calory from fossil fuel into the production of the food.

Another important factor in our rising interest in the sun is the realization of the great need for more mechanical power in the nonindustrialized areas of the world. Transportation and communication have improved so much that we are acutely aware, as never before, of the need to give technical help to our friends on the other side of the world and to the south of us.

I was privileged to attend a solar energy symposium a year ago in New Delhi, as a guest of the Indian Government and UNESCO. The Indian Government took us on a study tour of the semiarid regions. In one place we saw four bullocks and two men working diligently and skillfully for long hours irrigating farm land. Every minute, two bullocks pulled up by rope 250 pounds of water from a depth of 50 feet. One of the men collapsed the water bag made from hide and the water flowed over the land. The second operator drove the bullocks, and a second team of bullocks pulled up the next 250 pounds of water. This work done by the four bullocks and two men calculates out to be equivalent to one-third of a horsepower. And the four bullocks cost $600 and have a life of only about 6 years. Moreover, they have to eat and they must consume a considerable portion of the crops that they help to grow.

The project seemed to be barely self-supporting —but the water has to be raised. Of course, the bullocks are used for plowing and transportation as well as for irrigation. Now if the bullocks' walkway were covered with a solar heat collector that operated a crude engine operating at only 1-percent efficiency, a 1-horsepower pump could be operated. One percent efficiency is not much to ask for.

Elsewhere one could see a camel walking around in a circle pulling up water from a shallow well by a rotating mechanism. In a third method a man was getting water with a sweep and a counterweight and in a fourth a woman was walking back and forth on a balanced beam high above the ground to pull up pails of water.

The challenge to replace these primitive methods with solar engines seems more important to me now than some of the theoretical researches in which I have been engaged. Solar engines cannot compete yet with coal, oil, and electricity where these are abundant, but I believe that they can compete right now with such manpower and animal power.

High efficiency of the conversion of expensive fuel into work, and low labor requirements have been the technologic goals in industrialized countries. But sunshine is almost free, and so efficiency of conversion is less important. In nonindustrialized countries, low capital investment and small repair requirements are much more important than engine efficiency.

The need for new sources of power was brought out at the International Conference for the Peaceful Uses of Atomic Energy in Geneva last August. It seemed clear that atomic power can be produced on a large scale, in the future, for only a few tenths of a cent per kilowatt hour more than by coal-generated power. It seemed clear, too, that atomic power can be provided in comparatively small units very soon at a price considerably higher than that of coal-generated electricity. The price is not interesting in the United States, but it was interesting to some of the nonindustrialized nations. They pointed out that to build conventional power plants in remote, nonindustrialized areas requires large capital investments, such as the building of railroads and freight cars and engines and the installation of coal-mining machinery. Atomic power requires much less of this kind of capital cost, and so a higher price can be paid for atomic power. The same considerations apply to solar energy. Because no mines and no railroads, no wells and no pipelines are required, it is possible to pay more for atom-generated or sun-generated electricity than the industrialized countries are accustomed to pay for coal and oil-generated electricity.

The difference in labor costs between industrialized and nonindustrialized areas should be considered also in the development of solar energy. Adjusting mirrors to face the sun at frequent intervals and dusting them off could perhaps be done

for about $1 a day by a bullock-driver, turned solar-operator. Operators in industrialized countries might cost 10 times as much. Hand operation then may be less expensive and simpler than automatic operation in nonindustrialized countries, and it can permit a large reduction in the cost of the equipment. In places where electricity is not available, housewives would probably be willing to turn a wheel by hand at frequent intervals in order to preserve the family's food in a simple solar-operated refrigerator.

Still another factor in our awakened interest in solar energy is the development of new materials. Large areas of solar collectors are required for the utilization of solar energy, and large areas of any material are expensive. Machinery, metals, glass, and concrete cost thousands of dollars per acre. We live in an age of plastics, and thin plastics are now available which cost only hundreds of dollars per acre. Here is a new frontier where we may have a chance of cutting costs and making solar devices more economical.

There is much more to solar-energy utilization than the development of solar engines. House-heating, house-cooling, refrigeration, cooking, and distillation of salt water are all parts of the picture. Even in the development of solar power for operating machines, the long-range hope lies not so much in heat engines as in photochemistry and photoelectricity. But first let us see how much solar energy we need and how much we have.

Abundance of Solar Energy

In considering any new development, it is well to decide first whether it is theoretically possible, next whether it is technically feasible, and finally whether it is economically sound and socially useful. Answering the first question, the total energy of the sun falling on the earth is far more than is needed to do the world's work and it is an ever-continuing source of energy. To carry on the energy-rich civilization of the United States, in 1952 this country used 164,000 kilocalories of fuel energy or 186 kilowatt hours of heat per person per day. The per capita per day consumption of fuel by all the people of the world was 24,300 kilocalories in 1950. The total energy from the sun striking the land area of the United States is of the order of 270 million kilocalories, or 313,000 kilowatt hours, per person per day. The theoretically available supply of solar energy far exceeds the need, but, at present, there is no direct utilization of it, because it is in the form of low-temperature heat, which is difficult to convert into work and difficult to store and transport.

These figures are based on an arbitrary assumption that the solar radiation amounts to about 1 small calory per minute per square centimeter. It is more than this in Arizona and much less than this in some other parts of the country, particularly in the wintertime. This unit is too small to visualize. I have suggested a new unit of solar radiation—the "roof," which is a "million calories per minute". It is the solar radiation received by 100 square meters of flat surface at the rate of 1000 kilocalories per minute, or 60,000 kilocalories per hour, or 500,000 kilocalories or 580 kilowatt hours of heat per 500-minute day. This 100-square-meter area is 1075 square feet, about the flat area of the roof of a square house that is 10 meters, or 33 feet, on a side. A "roof-day" of 500,000 kilocalories is approximately equivalent to 2 million Btu, which is the amount of heat evolved by burning 150 pounds (about a man's weight) of coal or 15 gallons (almost an automobile tankful) of gasoline. If all the solar radiation were used with 100-percent efficiency, a "roof-day" of radiation would evaporate about 860 liters, or 230 gallons, of water.

If the solar energy could be converted into electricity through a heat engine and dynamo, or through a photovoltaic cell with an efficiency of 10 percent, 1 "roof" of radiation would generate about 7 kilowatts while the sun is shining at the rate of a million calories per minute. If this electric work is produced throughout 8 hours of sunshine but averaged over the whole 24-hour-day, the roof would produce at the rate of 2.3 kilowatts. A 5-percent efficiency would lead to 3.5 kilowatts during sunshine and 1.2 kilowatts for the day's average. The limits of 5 and 10 percent for conversion of heat into useful electricity is an optimistic but not an impossible range.

Social Implications

Solar-energy utilization holds out special hope for improving the standard of living in areas that have not yet become industrialized. In marginal agricultural areas, human labor can be conserved and the number of work animals with their high food consumption can be reduced, if the experts on solar-energy utilization are able to come up with a sufficiently practical and inexpensive solar engine. Pumping of water for agriculture and for household use and sanitation is the most obvious use of solar energy. There is no way to make the sun shine at night and the intermittency of solar energy is a powerful deterrent to its use. Water pumping is not handicapped by this intermittency. Electric lights for homes and villages might come next in importance, but for this purpose some system of

power storage must be provided, possibly for only a few hours. Village industries, weaving, wood turning, furniture-making, machine work, and radios might be encouraged with solar engines of 1 to 10 or 100 horsepower. Solar refrigeration could conserve food and make possible the use of additional protein materials with a resulting improvement in nutrition.

Solar heating and cooking are definite possibilities if the units are inexpensive enough.

I have a letter from Mexico telling me that the women of the village spend much of their time walking to the hills 6 miles away to collect firewood for their cooking and heating. The letter goes on to say, "we have plenty of sunshine all around. Can't you do something to help us?" Maybe we can. The challenge is not only to save time and labor for the housewife but to conserve the soil against erosion caused by removing the grass and shrubs for firewood. In some areas the cow-dung and camel-dung now used for cooking fuel might be saved for use as fertilizer.

Possibly the solar distillation of salt water and the solar heating and cooling of houses might open up new land areas to settlement and thus tend to relieve some of the world's population pressures.

In some undeveloped and uninhabited areas, a premium price could undoubtedly be paid for electricity, for pumping of water, and for distilled fresh water. In tackling the economic problems of solar energy, let us not be tied to the economic patterns and practices of the highly industrialized countries. There may be places where people would be glad to pay not 5 mils per kilowatt hour for electricity but 5 cents, because the whole economy and standard of living would be raised.

We must not become too enthusiastic, however. There are many places in the world with long, dark arctic winters or with cloud-shrouded climates where the sun cannot help. It is well to remember, though, that in a given area the heritage of sunlight is the same on all the land. The same amount of solar energy falls on an acre of land, whether it it is city real estate that costs thousands of dollars, rich farm land that costs hundreds of dollars, grazing lands that cost tens of dollars, or wasteland that costs almost nothing. In looking for large areas of unused land to be occupied with solar collectors, we might consider the rights of way at the sides of our railroads and highways—perhaps a "roof" equivalent for every 10 or 20 feet.

Solar energy is primarily for the countryside and not for the cities. When people are piled deep in multiple-family apartments and there is no vacant land, demands for power per square meter of sunlight cannot possibly be met. No wonder a report on solar energy by a committee of scientists in London was rather pessimistic.

Arizona and similar areas are favored places, with optimum sunshine throughout the year and plenty of great open spaces. They should be excellent proving grounds for industrial solar energy. The Bell solar battery has shown that we may expect rapid progress through fundamental research, perhaps in apparently unrelated fields.

Devices and Difficulties

In the space that remains, I want to give a hurried introduction to some of the opportunities that may lie ahead and some of the devices and difficulties with which we are all concerned. I hope that many new ideas for the utilization of solar energy will be brought forth that will lead to a chain reaction of additional ideas all over the world. I trust, too, that there are many experts who will evaluate these approaches to solar energy utilization technically and economically and criticize them relentlessly. There are plenty of fertile fields for solar exploration to be worked, and the few active workers should not ordinarily waste their efforts in unpromising areas. Early criticism may serve to direct our limited research resources into more useful channels.

Let me divide the future development of solar energy into short range and long range. About half of the radiation that we receive from the sun is visible light and half is heat. The light, of course, can be used as heat also. Thus all the energy is available as heat. We can start at once developing heat uses. The principles are well known. The difficulty is in economics. Some say "just wait for new technologic developments and new materials." The technologic ceiling will certainly rise. Yes, but it rises only because a few pioneers like to bump their heads against the ceiling. Let us start now on large-scale experimentation and pilot plants using the *heat* of the sun for heating, cooling, and distillation and for solar heat engines. These are needed right now in the nonindustrialized nations. For the long range, let us accelerate our fundamental research in trying to use the *light* of the sun. We are a long way behind in competing with the photosynthesis of agriculture, but agriculture in turn utilizes only a small fraction of the solar energy that theoretically it could utilize. The greatest long-range hope lies in new discoveries in photochemistry and in photoelectricity.

Solar Engines

Solar engines require a higher temperature than is ordinarily needed for house-heating, absorption

refrigeration, or distillation of salt water. This higher temperature can be achieved through concentration of the light onto a small area with the help of parabolic or parabolic-cylindrical mirrors; or by heat-traps with multiple glass plates that allow sunlight to pass through but minimize the loss of heat in the form of long infrared radiation from the heated receiver. The focusing type of engine has the advantage of reducing the area of the boiler, and this, in turn, reduces the heat losses and the cost of equipment. It is, however, nearly useless except in direct sunshine. The flat-plate collectors will continue some operation on cloudy days. The focusing type may well be more suitable for sunny climates, and the flat-plate collectors may be more suitable for regions in which the direct sunlight is frequently cut off by clouds. In each case, the important problem is to keep the capital cost low. Large areas of reflectors or glass plates are expensive.

Two attempts at inexpensive reflectors are interesting. A piece of thin transparent plastic is loosely stretched across a long rectangular frame above the ground and filled with water. The weight of the water produces a parabolic-shaped water lens, which gives a reasonably sharp focus on a pipe that can be moved along as the sun changes its level in the sky.

Several different types of parabolic mirrors have been constructed using aluminum foil and aluminized plastic. The least expensive one is made by placing 2-mil aluminum foil over a parabolic-cylindrical form and cementing coarse burlap cloth to it. Metal lath is draped over it and then coated thoroughly with a mixture of portland cement, sand, and water. After setting for a couple of days, the form with the inner coating of aluminum foil is lifted off and placed on the ground in an inverted position. The unit is 4 feet wide, the standard width of the aluminum foil and the metal lath, and 6 feet in effective length, with an actual length of 8 feet. The cost of materials is about 10 cents per square foot, and two men can make the unit quickly and lift it conveniently.

Thus far the reflector, which is ½ inch in thickness, seems to be strong and satisfactory. It is set at right angles to the orbit of the sun with a boiler pipe at the focus encased in a glass tube. It can be adjusted at intervals with a prop to keep it pointed toward the sun. Four rows each with ten of these reflectors, properly spaced, with a steam engine in the center, could give in principle 10 horsepower if the solar energy at 1 million small calories per minute were converted into work with a 10-percent efficiency.

Because the area is so large and the power produced of so little commercial value, it is necessary to use the greatest possible economy in building the reflectors. Even a wooden frame may cost too much. One of the least expensive ways to produce a parabolic mirror would be to scoop out in adobe or firm soil a long parabolic trough, parallel to the sun's orbit, and line it with aluminized plastic or sheet aluminum after proper treatment of the surface. Such an immovable reflector would be quite limited in its use.

Still other types of solar engines should be investigated for possible use when the capital costs must be kept to a bare minimum and where manpower is very inexpensive. A reflector may be moved in such a way as to focus the radiation first on one boiler and then on a second one. The steam from each boiler forces the piston in opposite directions, and thus it is possible to turn a wheel with the reciprocating piston. In another intermittent type a slotted screen allows alternate tubes in rows to become heated. They are connected to the cylinder, and the gas expanded by the solar heating moves the piston. By moving the slotted screen slightly, the hot tubes are now placed in the shadows and the other set of tubes in the sunlight, thus causing the piston to move in the opposite direction. By moving the shadow back and forth, the piston can be made to alternate its direction of movement and operate a wheel or diaphragm.

In designing inexpensive new engines, we should not be limited to old practices. Steam engines have been largely displaced by internal-combustion engines, and in trying to bring them back in 1955 we have available new materials and devices such as plastic diaphragms and sylphon bellows. Expansion and contraction of a plastic bag with check valves offers promise for the pumping of water. Solar-operated hot-air engines should be studied also, because they offer possibilities of simplicity and low cost.

Storage of Energy

Intermittency is one of the handicaps of solar energy, but the intermittency is usually regular and predictable. For dark hours there must be storage of energy. There are many ways in which mechanical or electric energy can be stored. The question is the economic cost. An engine can pump water up to an elevated reservoir, and, as the water falls back to ground level, it can do work in a water turbine. The difficulty with this method is the high cost of a reservoir that has appreciable capacity. One kilowatt hour of heat is equivalent to 4240 cubic feet of water, or 120,000 liters, falling through a height of 10 feet. If a hydroelectric plant, or an

abandoned mine, or a large diving bell in a lake or ocean is available, the cost may not be so great. At Austin, Texas, a steam power plant pumps water back from a low reservoir to a high reservoir when electricity is not much in demand, and then the full hydroelectric energy is available for peak loads.

Standard electric power grids are well suited to help overcome the intermittent nature of solar energy. When the sun is shining, the coal supply or the dammed-up water supply can be conserved. The demand for air-conditioning is creating new problems with the peak loads of electric power systems. The sun is usually shining brightest when the air-conditioning is needed most.

Energy can be stored also in the form of high-temperature heat. One kilowatt hour of heat is equivalent to 860 kilocalories, which is equivalent to a 10°C drop in the temperature of 86 liters of water. Heat to run a steam engine for a while after sundown might be stored in a large insulated hot-water tank or in fused material, such as urea with its melting point of 125°C and its heat of fusion of 50 calories per gram. Hüttig has proposed that heat for operating an engine be stored in an iron sphere heated electrically to a high temperature. Here the ratio of heat storage to radiating surface is comparatively low. It must be remembered that 1 kilowatt hour of heat gives only about 0.1 kilowatt hour of work in a solar engine.

The electrolysis of water and storage of the hydrogen and oxygen in underground gas tanks with water-seals offers possibilities. The gases may be combined to operate a high-temperature gas turbine. One kilowatt hour of heat is equivalent to the heat of combustion of about 9 cubic feet of hydrogen. The stored hydrogen and oxygen may be used directly to operate a hydrogen-oxygen "fuel cell" with nickel electrodes in a fused salt bath at a high temperature. Good laboratory progress is being made in obtaining efficiencies up to 60 and 70 percent with fuel cells.

House-Heating

House-heating is one of the theoretically simple uses of solar energy, because the temperature does not have to be high. Flat-plate collectors give satisfactory heat-traps, and focusing devices are not necessary. However, large surfaces are necessary, and architectural difficulties arise. The storage of heat may be accomplished with pebble beds, hot-water storage tanks, or chemicals that undergo fusion or transition in crystal form. Competition of solar heating against inexpensive coal, oil, and gas is difficult, but, inasmuch as about one-third of the fuel consumption in the United States goes for space heating, it is clear that there are important conservation aspects to solar house-heating. In northern climates, the capital cost of large heat storage capacities is so great that it seems better to add to the solar heating plant small auxiliary plants that use conventional fuel.

In the areas where fuel is expensive and wood and shrubbery should be conserved in order to minimize soil erosion, there is a special need for solar house-heating. In areas of greatest need, there is likely to be both a scarcity of fuel and an absence of electricity. What then is to be used in the solar heating plant for circulating hot air or hot water through the heat-storage bins? Circulation by natural convection is not usually enough. Efforts should be made to develop an inexpensive, solar-operated device for circulating air or water.

When ample electricity is available the use of a heat pump offers attractive possibilities with a reservoir of heat produced by solar radiation.

Refrigeration and House-Cooling

Household refrigeration is one of the most urgent fields for the utilization of solar energy. Nutritional deficiencies among people who live in tropical areas could be reduced if they had inexpensive refrigerators for preserving proteins and other foods. The potential market all over the world for refrigerators and house-cooling is tremendous.

In the United States with its abundant supply of electricity, refrigeration developments have followed the pattern of mechanical refrigeration with moving parts powered by electric motors. The absorption and desorption of ammonia in water and other similar types of refrigeration are probably simpler and more efficient. Intensive research on small solar-operated cooling systems should include not only absorption and desorption of gases in solutions but also adsorption and desorption of gases on solid surfaces. There should also be research on dehydration systems in which the dissolved water is driven out of a solution with solar heat and the dry high-boiling liquid is then ready to absorb more water vapor.

Distillation of Sea Water

A million calories per minute, or 500,000 kilocalories per day, of solar energy striking an area of 100 square meters could theoretically supply enough heat to vaporize about 1 million grams of water, which is a layer of water 1 centimeter, or 0.4 inch, deep. Practically, of course, the efficiency would be low, and solar distillation would usually correspond to less than 0.2 inch of rain. Multiple stills are possi-

ble in which some of the heat of condensation of the water is used to vaporize fresh water, but the cost of such stills is very high. Premium prices can be paid in some areas for domestic and household water and for drinking water for animals. Here the solar distillation of salt water is particularly attractive. Egypt, Africa, Australia, and parts of the United States have significant regions for practical tests.

Solar-distilled water cannot easily be provided for irrigation, because the investment costs of the necessarily large areas of water-containing vessels and glass or plastic are so high. There is need for new inexpensive, weather-resistant plastics that are impervious to water vapor but are wetted by liquid water and are able to withstand years of bright sunlight. Also needed is a solar-operated circulating fan that will circulate air where electric power is not available.

Photochemistry

The long-range hope for the direct utilization of solar energy lies in photochemistry and photoelectricity. The research goal in photochemistry is to find a suitable reaction that can be produced by sunlight with the absorption of energy and then be allowed to reverse itself at will in the dark with the evolution of energy. Many endothermic photochemical reactions reverse themselves so rapidly that they merely convert light energy into heat and are not suitable for storing energy. There is a chance that some of the photoproducts will give up electrons to a wire and thus produce an electric current. Most photochemical reactions are spontaneous reactions in which the light merely accelerates the reaction rate, and the reaction does not reverse itself in the dark. Again, many of the promising endothermic reactions respond only to ultraviolet light, which does not occur in sunlight to an appreciable extent. The photochemical challenge is a difficult one.

The fact that photosynthesis exists and carbon dioxide and water combine photochemically in the presence of chlorophyll in the growing plant gives encouragement to those who are trying to use the sun photochemically. The end-products of photosynthesis are carbohydrates and other organic materials that, on combustion, will give back the carbon dioxide and water and release the stored energy. The first chemical step in photosynthesis is the release of hydrogen atoms. In trying to copy and improve on nature, it may be easier to store the solar energy in hydrogen rather than in carbohydrates and other products produced by further reduction of the carbon dioxide. Although hydrogen cannot be used for food, it can readily be used for fuel.

Photosynthesis is a remarkable process, which we are just beginning to understand. In the laboratory under special conditions, it can be made efficient. Thirty percent of the light energy absorbed (corresponding to 8 photons per molecule) can be stored and released at a later time by combustion. In agriculture only a small fraction of 1 percent of the annual solar energy is stored. There are many reasons for this low efficiency. The half of the solar radiation that is in the infrared is ineffective; the growing season lasts for only a third of a year, and the ground is entirely covered with green leaves for only a part of this time; the carbon dioxide of the air is too low in concentration; and, most important of all, the sunlight is much too bright for maximum efficiency. As a result of all these handicaps, agriculture in the temperate zones of the world ordinarily grows only about 2 tons of dry organic material per acre per year.

The mass culture of algae can probably produce about 10 times as much organic material, but a heavy capital investment is required for water tanks, carbon dioxide enrichment, fertilizers, cooling equipment, and harvesting machinery. Research to reduce these costs should be encouraged. One of the best approaches is the development of algae that will grow in hot water and thus eliminate the need for artificial cooling.

The Bell Telephone Company's solar battery is one of the most hopeful developments. It was the result of fundamental research in solid-state physics —a field supposedly far removed from that of solar energy. The single crystals of silicon that form the battery are too expensive now, but intensive research should be encouraged in an attempt to find substitutes. Perhaps very inexpensive electroplated films and vaporized films of semiconducting material should be explored to see if they can be made pure enough and similar enough in properties to those of a single crystal. The single-crystal cell gives 10-percent direct conversion of the sun's energy into electricity without consuming any materials.

Conclusions

Solar scientists work in a heretofore neglected field with no adequate means of publication. Let us start now an international Journal of Solar Energy Research and Engineering. Science develops exponentially when its scientists can build on the work of others. Rapid publication of results is the life blood of a vigorous science. No single scientific journal is now in existence that is read alike by the many types of engineers and scientists who are

working on solar energy. Lack of a proper medium for publishing their research has been a deterrent to young scientists on the threshold of a professional career who might go into the development of solar energy.

The forthcoming International Geophysical Year is sponsored by the United Nations through ICSU, which in turn appoints committees and solicits support from national governments. Perhaps we can have a Solar Decade, if we want it. Shall we ask ICSU for an international committee on solar energy?

What can industry do to accelerate the utilization of solar energy? Among the most obvious aids would be mass production of large parabolic mirrors of aluminum pressed out with dies or of plastics with vaporized metallic films. Small, low-pressure steam turbines or steam engines, die-cast and produced in mass could help greatly to relieve human labor in some of the nonindustrialized areas. One- to 10- or 50-horsepower engines of sufficiently low cost could find an immediate and large foreign market with some domestic demand, even if the engines are quite inefficient. The development of thin plastics is needed for collectors of solar energy and particularly for the distillation of salt water. Special characteristics are needed such as wettability by water and opaqueness in the far infrared, but the chief demands are for low cost and ability to withstand years of exposure to bright sunlight.

Storage batteries at less than one-tenth of the cost of automobile batteries are needed, but they do not have to be small and portable. While laboratory research goes on for new ideas and new methods, studies of mass production of new materials and new machines by industry may well lead to rapid advances in the utilization of the sun's energy.

Solar-energy utilization is rapidly coming of age. Many people have become interested, and steady progress will be made. But do not expect miracles. We must go back to our laboratories and try to extract more value from our sunshine. It is there for us, if we can be smart enough to find it.

But I am confident that the scientists and engineers can and will bring a new era of prosperity and peace to the whole world. The International Conference for the Peaceful Uses of Atomic Energy opened up a great new hope for atomic power. Perhaps Arizona's "Sun for Man's Use" Conference will be just as important. The first half of our century may go down in history as the period of great wars, and it is not impossible that the second half of the century may come to be known as the beginning of a peaceful, power-abundant era in man's evolution. We know now that through research there is a chance that we can have mechanical power and electricity all over the world, and a greater equalization of industrial productivity in all countries may tend to lessen war tensions. I believe that, by a judicious combination of fossil fuels, atomic energy, and solar energy, the whole world can have within this century, all the mechanical power and material comforts that it wants. This development will not solve all the world's problems, because man does not live by kilowatts alone; but it will help.

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If you take from me my only sixpence, I have none left. . . . But if you take from me my only idea, you are (let us hope) enriched, but I am not impoverished; I may even acquire another idea in the process.—H. Dingle, "Science and modern cosmology," *Monthly Notices Roy. Astron. Soc.* **113**, 398 (1953); *Science* **120**, 513 (1954).

Acid-Base Terminology

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IN recent years clinical medicine has become concerned increasingly with the electrolyte structure of body fluids and the practice of electrolyte therapy. Unfortunately, and quite unnecessarily, no subject is more difficult for the medical student and the physician to comprehend. The difficulty derives not from any incomprehensible complexity of the physical and biological systems and mechanisms that are involved but rather from the incredible confusion and inconsistencies of terminology and definition that characterize textbooks and the medical literature.

The lack of understanding and the misunderstandings that follow from improper definitions of *acids* and *bases* have lately been emphasized again and forcefully by several authors (*1, 2*). To call cations "bases" and anions "acids"—as, for example, is done by Gamble in his otherwise excellent syllabus (*3*)—is exactly contradictory of elementary definition and description of acid and base properties. If acids are substances that furnish hydrogen-ion (proton, H^+) and bases are substances that react with or "neutralize" hydrogen-ion (proton acceptors), it is, of course, anions and not cations in body fluids as elsewhere that are a measure of capacity to react with acids and so comprise the *alkaline reserve* of the system.

When discussion of the electrolyte patterns of body fluids is extended to a consideration of "acid-base balance" and "alkaline reserve," precise and consistent definition of terms is essential.

Van Slyke and Cullen (*4*), in a classical paper that appeared nearly 40 years ago, proposed that *acidosis* should be defined as ". . . a condition in which the concentration of bicarbonate in the blood is reduced below the normal level." This is a straightforward, appropriate, and usable definition; it is appropriate inasmuch as bicarbonate reflects the residue of base that is available to neutralize acids stronger than carbonic acid and, thus, it can be taken clinically as a measure of the "alkaline reserve" of the body; it is usable and useful, because the concentration of bicarbonate in the blood has been made readily measurable by techniques and apparatus that have been developed by Van Slyke and his coworkers. Expressed as CO_2 capacity, the normal alkaline reserve of venous plasma may be taken as within the approximate range of 50 to 65 volumes percent. Acidosis would then describe any condition in which the CO_2 capacity was less than 50 volumes percent. Likewise, alkalosis would appropriately designate the status of the alkaline reserve when the CO_2 capacity was more than 65 volumes percent.

It is indeed very unfortunate that the terms *acidosis* and *alkalosis* have not been held to these specific and limited meanings. Instead, as Peters and Van Slyke (*5*) have noted, "The terms acidosis and alkalosis have been applied almost indifferently to two types of conditions: (1) states in which the bicarbonate content of the blood is altered; (2) states in which the true reaction or hydrogen-ion concentration of the blood is abnormal." It is completely inevitable that utter confusion and misunderstanding should have followed from such indiscriminate practice. If the terms *acidosis* and *alkalosis* are taken to describe the *quantity* of bicarbonate in the blood plasma, or any other system, they cannot intelligibly be used to describe also the hydrogen-ion concentration (pH) of the system. The pH of a buffer system is not a function of the absolute concentrations of the solutes, but of their relative concentrations. The pH of the blood plasma is not determined by the bicarbonate content (alkaline reserve), but by the ratio (molecular) of bicarbonate to carbonic acid. Thus, for example, if the blood plasma were diluted with an equal volume of water, the alkaline reserve (CO_2 capacity in volumes percent) would be halved without (appreciable) change in pH.

Since the terms *acidosis* and *alkalosis* cannot be employed rationally to describe both absolute and relative quantities, it is proposed that they be restricted specifically to their original meanings—namely, the status of the alkaline reserve as expressed in terms of the CO_2 capacity of the venous plasma.

Appropriate terms that have been proposed (*6*)

to describe abnormalities in the hydrogen-ion concentration of the blood are *acidemia* and *alkalemia.* If the normal pH range of venous blood plasma be taken as within the limits of 7.35 and 7.45, then *acidemia* would define a blood pH numerically smaller than 7.35 and *alkalemia* would define a blood pH numerically larger than 7.45.

The expression *acid-base balance,* which is scattered through medical literature, has been used with little or no understanding or with such vague or all-inclusive reference as to defy interpretation (*5*). If bases were cations and acids were anions, *acid-base balance* would be an expression without meaning, inasmuch as cations and anions must always be in balance with each other. Obviously, *acid-base balance* must relate to a limited and specific meaning, and the common-sense meaning would be the balance (ratio) existing between the proton donors and acceptors of the blood buffers, or, more simply, to the ratio between the carbonic acid and bicarbonate components. The acid-base balance of the blood would then relate to the pH of the system and would be described in such terms as *normal* (*compensated*), or *acidemia,* or *alkalemia.*

Since the alkaline reserve and acid-base balance may vary independently of each other, nine different combinations of the two are possible, as is represented in the following three paragraphs.

1) Normal alkaline reserve (CO_2 capacity = 50 to 65 volumes percent): (i) normal acid-base balance ($p\text{H} = 7.35$ to 7.45); (ii) acidemia ($p\text{H} < 7.35$); (iii) alkalemia ($p\text{H} > 7.45$).

2) Acidosis (CO_2 capacity $<$ 50 volumes percent): (i) compensated (normal acid-base balance: $p\text{H} = 7.35$ to 7.45); (ii) acidemia ($p\text{H} < 7.35$); (iii) alkalemia ($p\text{H} > 7.45$).

3) Alkalosis (CO_2 capacity $>$ 65 volumes percent): (i) compensated (normal acid-base balance: $p\text{H} = 7.35$ to 7.45); (ii) acidemia ($p\text{H} < 7.35$); (iii) alkalemia ($p\text{H} < 7.45$).

The maintenance of the normal status of the alkaline reserve and acid-base balance of the body is one of the most remarkable examples of the homeostatic capacities of the warm-blooded animal. The normally steady state represents an over-all equilibrium between the continuing depletion of the alkaline reserve by acid end-products of catabolism and the continuing replenishment of the alkaline reserve from food. Short-term fluctuations, which otherwise would occur continually and extensively with changes in metabolic rate and kind and with the periodicity of feeding, are minimized by the compensating activities of lungs and kidneys. The sensitive responsiveness of respiratory control and the ability of the kidneys to vary the pH and ammonia content of the urine are essential to avoid unphysiological changes in acid-base balance and to stabilize the alkaline reserve. It is only when these compensating capacities of lungs and kidneys are exceeded or impaired that an acidemia or alkalemia may exist. It is of more importance to the immediate welfare of the organism to avoid changes in acid-base balance than to maintain alkaline reserve.

Abnormal acid-base conditions of the blood can be referred to two types of *primary* disturbance; these, with some of the more *remote* causative pathologies are given in the following summary.

1) Metabolic type of acid-base pathology: Primary alkali excess may be (i) exogenous (administration) or (ii) endogenous (as in vomiting); primary alkali deficit from (i) diabetes mellitus, (ii) fasting, (iii) nephritis, or (iv) diarrhea.

2) Respiratory type of acid-base pathology: primary CO_2 excess from (i) impeded respiration, (ii) slowed respiratory exchange, or (iii) breathing air high in CO_2; primary CO_2 deficit from overbreathing, as with oxygen lack, hot baths or fever, congenital absence of sweat glands, encephalitis, hysteria, or drugs.

A brief discussion of several of the foregoing examples of acid-base derangements will suffice to illustrate the consistency and applicability of the terminology that has been employed here. It is to be noted that metabolic derangements in the acid-base status of the organism are always initiated by a change in alkaline reserve, with the development of acidemia or alkalemia following as a secondary or terminal event. Exactly the converse sequence of events characterizes derangements of the respiratory type; here the primary event is the development of an acidemia or alkalemia with subsequent compensation effected, if at all, by a matching change in alkaline reserve.

Primary alkali excess. As is indicated in the foregoing summary, a primary alkalosis may come about through the administration of alkali (exogenous) or through the neogenesis of bicarbonate in the body (endogenous). A primary alkalosis of endogenous origin is only an exaggeration of a normal physiological phenomenon that is associated with the secretion of hydrochloric acid into the stomach. Whatever may be the intimate details of gastric secretion, each molecule of HCl delivered into the stomach is matched by a molecule of bicarbonate added to the blood leaving the stomach. In comparison with the arterial blood to the stomach, the venous blood from the stomach may be said to exhibit both a relative alkalosis and alkalemia. A similar relationship exists between the renal arterial and renal venous blood when the

kidney is secreting an acid urine. These examples are in contrast to the acid-base relationships in the blood supply to a muscle area, where the venous blood plasma (mainly because of chloride-shift) has a larger alkaline reserve but a smaller pH relative to the arterial blood plasma.

Although the efferent blood from the stomach is promptly diluted with and buffered by other venous blood, its relative alkalemia is still effective in stimulating the kidneys to secrete a less acid urine (alkaline tide).

Clinically, endogenous alkalosis becomes important when excessive and continued vomiting (as in pyloric obstruction) prevents the normal reabsorption of the gastric juice. With the loss of chloride and water paralleling the continuing production of bicarbonate, the kidneys are presently unable to remove the excess of bicarbonate, which now needs to be retained as a substitute for chloride in supporting the osmotic pressure of the blood. Retention of CO_2 at the lungs is not adequate to compensate for the bicarbonate excess, and so the alkalosis is now associated with alkalemia. Parenteral administration of sodium chloride solution is designed to enable the kidneys to excrete preferentially the bicarbonate excess, thus restoring a normal alkaline reserve and acid-base balance.

Primary alkali deficit. A primary alkali deficit is, clinically, the most common and familiar example of acid-base pathology. The abnormally increased production of fixed acids from fat and protein that is consequent upon the inadequate carbohydrate metabolism of diabetes mellitus or fasting cannot feasibly be compensated by the administration of alkali. The resulting acidosis, however, may remain compensated (normal pH) until the CO_2 capacity has reached quite low values. This compensation is effected both by the increased elimination of CO_2 at the lungs and by increased acidity and ammonia content of the urine. Only when lungs and kidneys are unable longer to keep pace with the reduction in alkaline reserve does acidemia supervene.

Primary CO_2 excess. A primary CO_2 excess results whenever the rate of elimination of CO_2 at the lungs is less than the rate of its production in the body. Among the familiar causes of CO_2 retention are the slowed respiration induced by barbiturates or morphine and the breathing of air with a CO_2 content larger than that of normal alveolar air. It is obvious that the retention of carbonic acid does not have any *direct* or *immediate* effect on the alkaline reserve (bicarbonate), but only on the ratio of base to acid. Accordingly, the initial change is in the acid-base balance and is to be termed acidemia, not acidosis. With normal kidney function, compensation will be undertaken—that is, the acid-base balance will be restored to its normal pH range, through the secretion of a more acid urine, the production of more ammonia by the kidneys, and the formation and retention of bicarbonate. Thus may an initial acidemia become a compensated alkalosis.

Primary CO_2 deficit. Overbreathing, induced by oxygen lack (as at high altitude), hysteria, drugs, or other factors, creates a primary CO_2 deficit and thus an alkalemia. Initially there is no change in the alkaline reserve. However, the primary derangement in acid-base balance stimulates the kidneys to excrete the *relative* excess of bicarbonate, thus creating a compensated acidosis. It is worthy of note that administration of alkali is contraindicated in this situation.

Summary. (i) Much confusion and misunderstanding in discussions of the alkaline reserve and acid-base of biological systems would be avoided by adopting and employing a consistent and precisely defined terminology. (ii) Since the alkaline reserve and acid-base balance may vary independently of each other, and even in opposite directions, they cannot be described in the same terms. (iii) It is proposed that the alkaline reserve be described in terms of *normal, acidosis,* and *alkalosis,* which have reference to the CO_2 capacity of the venous plasma. (iv) It is proposed, further, that the term *acid-base balance* designate only the molecular ratio existing between the bicarbonate and carbonic acid components of the venous blood plasma and so be expressed as the pH of the plasma. It is suggested that the terms *acidemia* and *alkalemia* be employed to describe the acid-base balance when the pH is, respectively, numerically smaller or larger than normal.

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Radioactive Methods for Geologic and Biologic Age Determinations

OTTO HAHN

Dr. Hahn is president of the Max Planck Society for the Promotion of Science. In 1945 he was awarded the Nobel prize in chemistry for the year 1944. This article is a translation of a lecture presented at last year's meeting of Nobel laureates at Lindau, Germany. It was first published in German in Naturwissenschaftliche Rundschau [8, *331 (1955)*] *and is reprinted here with the permission of the publishers. We are indebted to Mrs. A. Akeley of the department of physics, Purdue University, for the English translation.*

A NUMBER of years ago I suggested a then new method, called the strontium method, for determining the age of minerals and at the same time the geologic age of the layers in which these minerals occur. This method has proved fruitful. It is used, and it has been used to a great extent in the United States. In the meantime, other methods have been added, particularly the potassium-argon method. Lately, biological ways have also been found for determining the very latest earth history—the time during which human beings enter our historical consciousness. Finally, methods have been found that enable us to fix dates almost up to the present day.

I would like to give a summarizing report about these methods. Because I am personally connected with its development, I should like to discuss the strontium method in somewhat greater detail than the other methods, which I can only review.

Since the rate of the radioactive transformation processes that occur in uranium and thorium after their crystallization cannot be influenced, these transformations offered the first accurate method of determining the age of different geologic periods of the earth. The element uranium changes gradually, through a series of transition products—the most important of which is radium—into its inactive end-product, the lead isotope with an atomic weight of 206. The transformation rate is extremely small; in any uranium mineral, only 1 percent of a given amount of uranium is transformed in 65 million years.

Similarly, but even more slowly, thorium is transformed through a number of active intermediate products into the stable lead isotope of mass 208.

An accompanying phenomenon of this lead-producing process is the emission of α-particles or helium nuclei. These particles lose their initially very high speed as well as their charge very soon when they pass through solid matter, and they remain trapped in the mineral in the form of ordinary helium gas.

The older methods of geologic age determination were based on the determination of the lead and helium content of uranium and thorium minerals. The amount of lead isotope of mass 206 in uranium minerals enables one to determine the duration of the lead-forming process in the uranium mineral. From the age of the mineral so determined, one can deduce the age of the geologic period in which the mineral was crystallized. In the helium method, similarly, the amount of helium produced in the mineral is determined.

Both methods have been very successful compared with older, nonradioactive methods. However, with increasing geologic age of the investigated minerals, the helium method shows steadily increasing deviations in value compared with the value obtained from the lead content: a considerable amount of helium escapes from the mineral, and the age determined from helium is too low. This is not astonishing because a geologically old uranium mineral may contain 20, 30, and even more cubic centimeters of helium per gram of uranium. The gas inside the mineral is therefore under very high pressure and attempts to escape wherever it can.

However, for very old minerals, the lead method, also, is not free from possible errors. In such minerals, more than 1000 million years old, more than 20 percent of the original uranium content has been transformed into lead. It must be feared that a so-considerably altered crystal structure underwent changes, even though this is not yet outwardly recognizable. It may be that part of the lead was leached out; then the age determined would be too low. However, it is also possible that uranium was removed; then relatively too much lead would be found, and the age determined would be too high.

Strontium Method

It follows that reliable lead values can be expected only from specially selected, dense mineral samples that are weathered as little as possible. Considering that the age determination of the earliest periods of our earth history is of special interest, I asked myself years ago if it might not be possible to use the formation of the strontium isotope of mass 87 that is formed from the β-radiating rubidium of mass 87 to determine the age of rubidium-containing minerals.

It has been known for a long time that potassium as well as rubidium possesses natural radioactivity. Emitting β-rays, these elements are transformed into calcium and strontium.

Let us consider first rubidium and its daughter-product strontium. If one knows the transformation rate of radioactive rubidium, one may in principle proceed with the age determination of a rubidium-containing mineral in the same way as he does in the age determination of a uranium-containing mineral, with the afore-mentioned lead method. It is necessary to determine how much rubidium the mineral contains and also to determine the amount of strontium-87 that has been formed from radioactive rubidium-87.

In order to calculate the age (Fig. 1) from these data, we refer to the basic law of radioactive decay. It states that if the number of rubidium-87 atoms at the time of crystallization or solidification ($t = 0$) is N_0, then at a later time t, this number is $N = N_0 e^{-\lambda t}$.

The number N' of the strontium-87 atoms produced is therefore given by

$$N' = N_0 - N = N \quad (e^{\lambda t} - 1).$$

The amount of strontium-87 formed is still small compared with the amount of its mother substance, rubidium-87, even in the geologically oldest minerals. Therefore, $N'/N \ll 1$. One is able to develop the exponential into a series and limit it to the first term. One obtains:

$$N'/N = 1 + \lambda t + \ . \ . \ . \ - 1 = \lambda t$$

For calculations of age, one therefore has the simple formula

$$t = 1/\lambda \cdot N'/N = 1/\lambda \cdot Sr^{87}/Rb^{87}$$

Therefore we need now the amount N' of strontium-87 formed inside the mineral and the amount N of rubidium-87 not yet transformed into strontium-87. Analysis of the rubidium in the mineral yields N. From the total content of the mixed element rubidium, one knows the content in radioactive isotope rubidium-87; it is 27.85 percent of the total amount.

$N = N_0 \cdot e^{-\lambda t}$ N_0 = Number of Rb atoms at a time $t = 0$
N = Number of Rb atoms at a time t
λ = Decay constant of Rb^{87}
N' = Number of Sr atoms produced
$N' = N_0 - N = N(e^{\lambda t} - 1)$
$N'/N = e^{\lambda t} - 1$
since $N'/N \ll 1$, one can write
$N'/N = 1 + \lambda t + \ . \ . \ . \ - 1 = \lambda t$
For the age, one then obtains the formula
$t = 1/\lambda \cdot N'/N$

Fig. 1. Age determination if the transformation rate is known.

This amount has therefore been transformed into strontium-87, which we call "radiogenic" strontium. However, it is not necessary that all the strontium the mineral contains is radiogenic—formed in the mineral. It might also be ordinary strontium that was orginally contained in the mineral, precisely as a uranium mineral may contain not only radiogenic lead but also ordinary lead.

The ratio between radiogenic strontium and total strontium, or Sr^{87}/Sr, can be found exactly by mass spectroscopy. Then we have all the data needed to determine the age t.

The important part of this determination is therefore the mass-spectroscopic analysis of strontium. That such an analysis is entirely possible can be shown by two pictures of mass spectrograms that were taken a number of years ago by Josef Mattauch of the Max Planck Institute for Chemistry. Figure 2 is an earlier picture. On top is a mass spectrogram of ordinary strontium; underneath are three further spectrograms showing increasing amounts of radiogenically formed strontium from different old rubidium-containing minerals.

The lower spectrogram represents a lepidolite, a lithia mica that I got in 1933 from Canada. This spectrogram shows that this mineral did not contain ordinary strontium in detectable amounts, for the strongest mass line of natural strontium, the isotope of mass 88, is not recognizable. Very strong, however, is the line for strontium-87, which in natural strontium is found to the extent of only 8.5 percent.

A few years later Mattauch was able to take much sharper mass spectrograms (Fig. 3) with an improved spectrograph, using a number of rubidium-containing minerals. My coworker at that time, Strassmann, now professor in Mainz, performed the chemical separation of the always very small amount of strontium. The order of the pictures is this time reversed. This time the lowest spectrogram shows the ordinary isotopic mixture of strontium. The ratio between the content of

strontium-87 to the total amount of isotopes is always 0.085 to 1.

The content of radiogenically formed strontium increases continuously in the mineral samples listed from the bottom to the top. In the following six Swedish minerals, the content of strontium-87 increases from 1.13 to 46. The three top samples from northwestern Canada show a still higher content of radiogenic strontium. In the topmost sample, the strontium content rises to more than 360. Therefore, we see an increase in intensity of the radiogenically formed strontium-87. On the other hand, the intensity of the lines for ordinary strontium, which is most strongly represented by the isotope of mass 88, is steadily decreasing. Natural strontium moves into the background and disappears completely in the top picture. A comparison with the lines for bromine and tungsten, which are also on the plates, is interesting. The lines differ from picture to picture but show no shift in frequency.

The column farthest to the right of the spectrograms shows the content of radiogenically formed strontium. Ordinarily, regular strontium does not contain radiogenically formed strontium. In the top sample, the content of radiogenically formed strontium is more than 99.7 percent. This again corresponds to the lepidolite that I got from Canada

Fig. 2. Mass spectrograms of strontium bromide. Origins: (1) ordinary $SrBr_2$; (2) pollucite, Varuträsk, Sweden; (3) microcline, Varuträsk, Sweden; (4) mica, Great Bear Lake, Canada.

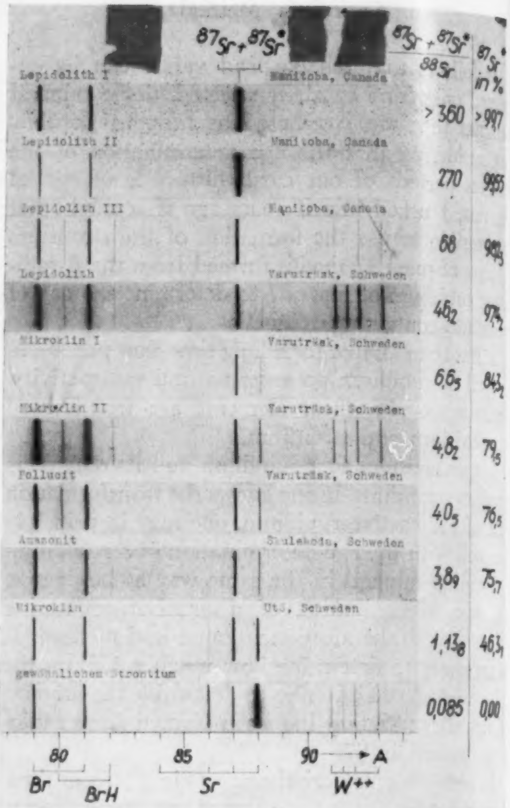

Fig. 3. Mass spectrograms of strontium bromide samples. The name of the mineral is given at the left of each spectrogram, and the locality at the right. The extreme right-hand column shows the content of radiogenically formed strontium.

some time ago. You have already seen its spectrogram (Fig. 2).

However, the age of these different minerals is not given. Influences of the war and the shift from our bombed-out institute to a new, provisional place made it impossible to determine analytically the rubidium and strontium content of the samples.

The strontium method was, however, taken up by others, especially by the American scientists Ahrens and collaborators and Aldrich and coworkers. Instead of the difficult analytic chemical analysis, Ahrens used the spectrochemical method. He discussed his procedure in detail. Table 1 shows part of the results of an investigation by Ahrens and MacGregor (*1*) of geologically very old lepidolites from South Africa. A total of nine samples was investigated. The results, not reproduced here, were the same. On the average, an age of 2100 million years was found. Ahrens and MacGregor did not, however, perform a mass spectroscopic analysis in their determination, and they assumed that the investigated mica does not contain ordinary stron-

tium. This might be justified in the case of these lepidolites. But it is certainly not justified for all rubidium-containing minerals. In the summary made by Mattauch, we have seen in the cases of a few microclines (feldspar) that they contain not only radiogenic strontium—therefore strontium from rubidium—but also ordinary strontium. Therefore, in general, a mass-spectroscopic determination of strontium cannot be avoided.

A completely different method of determining analytically the rubidium as well as the strontium in such minerals is the isotope-dilution method. This method was previously introduced by Rittenberg and his collaborators in biochemical experiments. It is used now by Aldrich for age determinations according to the strontium method (*2*).

This is the method. Let us say a chemical element contains two isotopes. Through mass spectroscopy, their normal percentage is known. If a known amount of one of the isotopes is added to the element, a further mass-spectroscopic investigation shows the dilution which the added isotope undergoes through the isotope mixture to be analyzed. If the samples with the added isotope were homogeneously mixed before analysis, no analytic chemical weight determination is necessary because possible losses during the separation of the element influence the added amount of isotope to the same degree.

After a known amount of rubidium-87 has been added to the mixed element rubidium-85 and -87 in the mineral, a mass spectrogram will show the dilution of the added pure isotope by the isotope mixture. In this way the rubidium content of the mineral sample can be found. Aldrich made in a very similar manner an analysis of the strontium that the rubidium mineral contains. For this purpose, strontium-84, contained to ½ percent in ordinary strontium, is the added isotope.

This much more accurate and simpler analysis of the isotope-dilution method is made possible through the stable isotopes obtainable in the United States and in England; thus the rubidium-strontium method yields very reliable values indeed. For this method, however, a knowledge of the transformation rate of rubidium into strontium is necessary. The final decision regarding the half-life has yet to be made.

I myself, I think as the first, determined the half-life of rubidium more than 40 years ago. I found, using a still very primitive method, a half-life of 5.5×10^{10} years for the active isotope rubidium-87. New values, determined by different investigators, are between 5.8 and 6.0×10^{10} years. The afore-mentioned age found by Ahrens is based on a median value of 5.9×10^{10} years. Huster and co-

Table 1. Strontium age determinations on nine lepidolite specimens from Southern Rhodesia.

Location		Age (10^6 yr)
Pope Tantalum Mine, 11 miles east of Salisbury (large mauve flakes)		2000
		1900
		2500
		2100
		2350
	Mean	2150 ± 200
15 miles NNE of Salisbury (pale, compact)		2300
		2200
		1750
		2700
		2200
	Mean	2200 ± 200
Lutope Tin Mine, Wankie District (deep purple, medium-grained)		2050
		2100
		2200
		2100
		2100
	Mean	2100 ± 200
Odzi District (light mauve, medium grained)		2000
		2000
		1950
		2450
		1950
	Mean	2100 ± 200

workers have shown in recent times that apparently all measurements made so far with counters have to be corrected (*3*). They found a value of only 4.3×10^{10} years. The 2100 million years found by Ahrens have to be reduced in a ratio of 4.3/5.9 according to this new information. The reliability of the method itself is not, however, impaired.

Potassium-Calcium Method

Potassium too contains an active isotope. Its mass is 40, and it emits β-rays. It is therefore transformed into calcium-40. The mixed element unfortunately contains only 0.012 percent of this active potassium. In contrast to this, the mixed element rubidium contains 27.8 percent active rubidium. However, in the very long geologic periods, due to the not very long half-life of active potassium—1.3×10^9 years—a certain but small amount of radiogenic calcium is formed. Unfortunately, calcium-40 is the most frequent partner of the regular mixed element calcium. Therefore, only in very old potassium minerals, nearly completely free of calcium, is it possible to find through extremely accurate mass spectroscopy the very small shift in the isotope ratio of calcium, and thus

use the activity of the potassium for age determination. Ingram and coworkers have actually detected through mass spectroscopy the very small amount of radiogenic calcium in very pure and calcium-free potassium minerals. Ahrens has discussed the possibility of making age determinations with this calcium method in special cases of very old minerals (*4*).

Potassium-Argon Method

When dealing with potassium, one finds that there is another possibility that is even more promising. Not only β-rays are emitted when potassium forms the next higher element calcium, but potassium also forms through K-capture (electron capture into the nucleus) the next lower element, argon.

Figure 4 shows the disintegration scheme of potassium. The branching disintegration scheme shows that the K-capture, the capture of an electron from the K-shell of potassium into the nucleus, is followed by gamma radiation. Twelve percent of the total transformation is to argon-40, 88 percent to calcium-40. It was found experimentally that 3.3 γ quanta are emitted per gram of potassium per second, and therefore the same number of argon atoms is produced. If the very sensitive methods available for detecting small amounts of argon are used, this produces, after many millions of years, traceable quantities.

The argon-isotope mixture present in air consists, unfortunately, of 99 percent of this argon-40. Therefore, in argon determinations from potassium minerals, pollution through argon from the air has to be avoided very carefully. Mass-spectroscopic or mass-spectrometric analysis of the argon permits one to detect such pollution by regular argon. Another source of error is the diffusion of

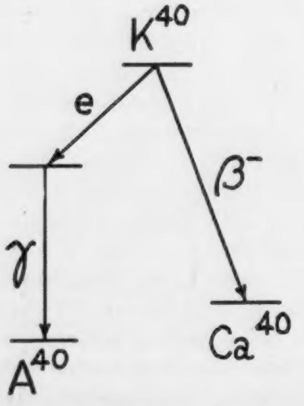

Fig. 4. Disintegration diagram for potassium-40.

the argon from potassium salts, which is a function of crystal size (as with helium out of uranium minerals). With these sources of error carefully considered, Gentner and his students made a number of age determinations, taking into account the diffusion and determining the purity of the radioactively formed argon (*5, 6*).

Tables 2 and 3 reproduce some of the results of Gentner and his collaborators.

Figure 6 shows the age determination of some minerals, mostly potassium feldspars from the Upper Carboniferous. The first value of 254 million years, compared with the later, somewhat higher values, is according to Gentner beyond the experimental error. The Waldkircher pegmatite is therefore younger than the other samples. The microcline from Varuträsk was examined for its age by both the argon and the strontium methods. The agreement is satisfactory, considering that the strontium values are too high, probably because the half-life of rubidium was taken too high.

Figure 7 shows how well the age values agree for two potassium salts from the same geologic period but of different origin.

Not only Gentner and his students, but also Fritze and Strassmann and quite recently Noddack and Zeitler have used the argon method for age determination, and they have obtained some preliminary results.

Comparison of Methods Mentioned So Far

In comparing the rubidium-strontium method with the potassium-argon method, we can say the following. It is possible to make safely and easily an analytic determination of rubidium as well as the strontium produced. Such an analytic determination can be performed with the isotope-dilution method if a mass spectrometer is available. The strontium method does not, therefore, offer any difficulties in principle. Using this method, one gets reliable results if the half-life of active rubidium is known. This method has the disadvantage that not many minerals exist that are fairly rich in rubidium and in which enough radiogenic strontium has been formed for analytic detection. This method is, therefore, safest when it is used on geologically very old minerals.

The potassium-argon method is not limited to comparatively few and geologically old minerals. Potassium is one of the elements found most often in the crust of the earth. One has therefore a wide choice in all age groups. Its disadvantage is the low content of active potassium in the mixed element. Therefore, only a small amount of argon is formed, which has the same mass as the argon

which is found abundantly in air. It is therefore necessary to avoid pollution by natural argon. It is necessary to make a mass-spectrometric analysis of argon of mass 36 of the air that is in the gas sample. It is also necessary to know the diffusion of the radiogenic argon from the mineral, which is different for different minerals. By paying attention to these complications, one will be able to get wide applications of the argon method.

The report so far shows that in principle the strontium and the argon methods are not different from the classical lead and helium methods. They use the inactive transformation products that are formed at a known rate from the long-lived and naturally present elements rubidium and potassium. The strontium-argon methods have advantages compared with the lead-helium methods. However, they need further application and maybe further improvement.

Carbon-14 Method

Compared with the methods mentioned so far for geologic age determination, the methods still to be discussed can be called biological methods. These biological methods do not determine the transformation products of slowly decaying elements. They do, however, determine the content of a radioactive atomic species produced in nature and disintegrating comparatively fast. It is the "active" carbon, the carbon isotope of mass 14 that is constantly produced in our atmosphere by cosmic rays. This method was developed by the American physicist W. F. Libby, was worked out by him to high precision, and gave extremely interesting results (*7*). The method is based on the following: active carbon is produced when cosmic rays hit nitrogen in air according to the reaction ${}_7N^{14}(n,p){}_6C^{14}$; the carbon-14 emits β-rays and disintegrates into the original nitrogen with a half-life of 5600 years. The number of active carbon atoms is practically equal to the number of neutrons formed by cosmic radiation. This amount was found to be 2.4 atoms per second, per square centimeter of the earth's surface. The active carbon changes to carbon dioxide, is mixed with ordinary carbon dioxide from the atmosphere, and is then distributed by different life processes over the whole biosphere. The specific activity of carbon on the earth's surface was calculated to be about 17 disintegrations of carbon-14 atoms per minute, per gram of carbon; actually found were 15 to 16 disintegrations.

When a living creature dies, the assimilation or absorption of organic foodstuff, consequently the absorption of carbon-14 from the atmosphere, is interrupted, and the specific activity of carbon is constantly reduced by radioactive decay. On samples of *known* age chosen by an American committee of specialists, the usefulness of the carbon-14 method for age determination was tested. Considering the background of the counter, it is indeed very difficult to take measurements of the very small carbon activity, for the background interferes with measurements of such small activity. The background is of twofold origin: (i) disturbances by traces of uranium and thorium; (ii)

Table 2. Age determination on different minerals by the argon method.

Material	$\frac{mm^3 \text{ rad } A^{40}}{g \text{ potassium}}$	Age (10^6 yr)	Geologic formation
Waldkirch (Black Forest) pegmatite	1.055	254	Upper Carboniferous
Albtal granite, Tiefenstein, Black Forest	1.142	274	Upper Carboniferous
Potassium feldspar; large crystals from albtal granite	1.212	289	Upper Carboniferous
Granite porphyry, Black Forest	1.178	281	Upper Carboniferous
Potassium feldspar from granite porphyry Geisbaumhalde	1.17	280	Upper Carboniferous
Varuträsk, North Sweden, Microcline	8.49	1.45×10^9	Pre-Cambrian
Microline	(Strontium method)	1.6×10^9	Pre-Cambrian

Table 3. Age determination in salt deposits from the Tertiary (Lower Oligocene, *2*). Argon method.

Material	$\frac{mm^3 \text{rad } A^{40}}{100 \text{ g Potassium}}$	Age without consideration of difference loss (10^6 yr)	Age with difference correction (10^6 yr)	Age with difference and temperature correction (80° to 40° C) (10^6 yr)
Sylvinite from potassium mine, Buggingen, Upper Rhine lowland	6.90	18	22	25
Anna Mine, Alsace	6.90	18	22	25

mesons formed in the atmosphere by cosmic radiation.

A thick iron casing protects against disturbances from radioactivity (according to Libby, lead is too often radioactively contaminated). Mesons, however, penetrate this protective casing. In order to eliminate the mesons too, the main counter is surrounded by a circle of protective counters. They are connected in such a way that the main counter does not respond if any of the protective counters respond at the same time. The μ-mesons pass through the whole circle of counters but are not recorded by the main counter because a protective counter always responds also. In this way it is possible to reduce the background to 4 counts per minute.

For activity determinations, about 8 grams of carbon are distributed in a thin layer on the inside wall of the counter. After subtracting the background, one observes under these conditions 6.7 counts per minute from freshly liberated carbon that originated from carbon-14. Measuring for 48 hours, it is possible to determine an age of 5000 years with an accuracy of $\pm$ 300 to 400 years.

Table 4 shows some of the most important results of Libby's work. Considering that carbon-14 has a half-life of 5600 years, 30,000 years was taken as the upper limit for possible determinations, for only a minute amount of carbon remains after that time. The lower limit is probably 1000 years, for below 1000 years the deviation from the zero activity of the carbon becomes too small. There is, however, no doubt that, within these limits, this method of age determination for biological or archeological objects will find wide application.

Table 4. A few examples of age determinations made with the C^{14} method.

Sample and location	Age (yr)
1. Age of the find in the Lascaux Cave, southern France (mural paintings)	15,000
2. Remains of a campfire in the Hazer-Merd Cave, Iraq	25,000
3. Many shoes from a cave in Oregon	9,000
4. The first pyramids in Egypt	4,800
5. Pile dwellings on lakes in Switzerland	5,000
6. The Stonehenge-Monument in south England	3,700
7. Age of a Japanese canoe	3,000
8. Men on the Hawaiian Islands only since	1,000

Tritium Method

As a *Kuriosum,* let us look at a way which in principle permits us to determine, according to a radioactive method, the age of objects that are not so old. In question is the extra-heavy hydrogen, tritium, which decays with a half-life of 12 years. Neutrons that are produced by primary high-altitude radiation form not only carbon-14 but, even though to a much lesser degree, tritium also. It seems that the following reaction takes place:

$$_{7}N^{14}(n,T)\ _{6}C^{12}$$

this process is, however, not yet fully understood. Investigations using the great accelerators in the United States seem to confirm the assumption about the origin of tritium.

The presence of natural tritium was detected and measured in rain water in the following way: the heavy isotopes of hydrogen are enriched by electrolysis. From previous experiments, the ratio of factors for enrichment of deuterium and tritium is known. The content of tritium in the enriched water is measured with a counter. The water vapor is decomposed over zinc. The resulting hydrogen is used with a little argon and ethylene as gas for the counter. In this way it is possible to detect even one tritium atom in 10^{19} atoms of normal hydrogen.

Libby arrived at the conclusion that the average tritium production through cosmic radiation is 0.14 tritium atoms per square centimeter, per second (compared with 2.4 carbon-14 atoms). The tritium content of rain water of different origins was determined. There is a difference between rain over the large inland expanse of the American continent and rain over the ocean. The tritium content in water depends on the time in which the water vapor, after evaporating from the ocean, is exposed to cosmic radiation, which in turn forms tritium. For water of the some origin, about the same tritium content was found. In this way, it is possible to check approximately the age of famous wines. If a so-called Napoleon wine really originated from the time of Napoleon, it contains no tritium. If the wine is 12 years old, it has half the tritium content of a wine of 1955 vintage.

Obviously such statements must be made with great care because of the infinitely small amount of tritium in ordinary rain water. However, this method offers, without doubt, possibilities for hydrology. It enables one to decide whether a spring is supplied by fresh rain water or ground water many years old. It is only necessary to find out whether tritium is in the water or not. Certain Italian hot springs originate from old times. However, the water of Hot Springs, Arkansas, is surely modern rain water.

Thus we have seen that today there exists a number of possibilities for exploring, although not without gaps, by radioactive methods, the history of our solid earth crust from its earliest periods up to our days.

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ASSOCIATION AFFAIRS

AAAS Headquarters

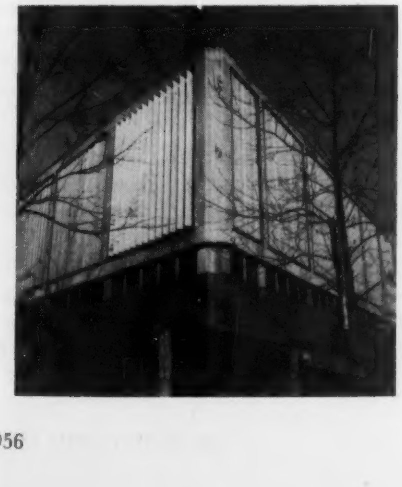
The new AAAS headquarters will be ready for occupancy late this spring. The Association will use the first two floors of the building, and the third floor will be shared by several affiliated societies.

The architects, Faulkner, Kingsbury, and Stenhouse, have designed the building with unusual attention to thermal controls. Three sides of the structure have two-story aluminum louvers covering the windows. These provide good natural light and, at the same time, reduce the cost of air-conditioning by deflecting the sun's rays.

The louvers are operated by an electric motor and a clock mechanism; the angle of the aluminum panels changes with the movement of the sun. The timing device will probably have to be adjusted about six times a year so that the windows will always be shaded. The louvers have created considerable interest, for, although they have been used on the West Coast, this is the first time that they have been tried in the Washington area.

All mechanical equipment for the new structure is enclosed in the penthouse—for example, the air-conditioning and heating units and the elevator shaft.

The two photographs, showing the back corner of the building at 15th and N Streets and the front corner at 15th Street and Massachusetts Avenue, were taken by Kenneth Gilmore of the *Washington Daily News.*



AAAS Socio-Psychological Prize

Through the generosity of an anonymous donor, the AAAS offers an annual prize of $1000 for a meritorious essay in socio-psychological inquiry. The conditions of competition for the prize to be awarded at the 1956 annual meeting, New York, 26–31 Dec., are as follows.

1) The contribution should further the comprehension of the psychological-social-cultural behavior of human beings—the relationships of these hyphenated words being an essential part of the inquiry. Whether the contributor considers himself to be an anthropologist, a psychologist, a sociologist, or a member of some other group is unimportant as long as his essay deals with basic observation and construction in the area variously known as social process, group behavior, or interpersonal behavior. For ease of reference in the rest of this statement, this general area will be called "social behavior."

2) The prize is offered to encourage studies and analyses of social behavior based on explicitly stated assumptions or postulates, which lead to testable conclusions or deductions. In other words, it is a prize intended to encourage in social inquiry the development and application of dependable methodology analogous to the methods that have proved so fruitful in the natural sciences. This is not to state that the methods of any of the natural sciences are to be transferred without change to the study of social behavior, but rather that the development of a science of social behavior is fostered through observation guided by explicit postulates, which in turn are firmly grounded on prior observations. It may be taken for granted that such postulates will include a spatial-temporal framework for the inquiry. It may properly be added that the essay should foster liberation from philosophic-academic conventions and from dogmatic boundaries between different disciplines.

3) Hitherto unpublished manuscripts are eligible, as are manuscripts that have been published since 1 Jan. 1955. Entries may be of any length, but each should present a completed analysis of a problem, the relevant data, and an interpretation of the data in terms of the postulates with which the study began. Preference will be given to manuscripts not over 50,000 words in length. Entries may be submitted by the author himself or by another person on his behalf. Each entry should be accompanied by four copies of an abstract not to exceed 1200 words in length.

4) Entries will be judged by a committee of three persons considered well qualified to judge material in this field. The judges will be selected by a management committee consisting of the chairman and the secretary of Section K and the executive officer of AAAS. The committee of judges reserves the right to withhold the prize if no worthy essay is submitted.

5) Entries should be sent to Dael Wolfle, Executive Officer, American Association for the Advancement of Science, 1515 Massachusetts Avenue, NW, Washington 5, D.C. Entries should be submitted in quadruplicate. The name of the author should not appear anywhere on the entry itself but should be enclosed on a separate sheet of paper which also gives the author's address and the title of his essay. To be eligible for consideration for the prize that will be awarded at the 1956 annual meeting of the Association, entries must be received *not later than 1 Sept. 1956.*

AAAS Sections Call for Papers for the New York Meeting

Nine sections of the Association will arrange sessions for contributed papers at the New York meeting, 26–31 Dec. 1956. The secretaries to whom titles and brief abstracts should be sent, *not later than 30 Sept. 1956*, follow:

C–Chemistry. Dr. Ed. F. Degering, 26 Robinhood Road, Natick, Mass.

E–Geology and Geography. Dr. Robert L. Nichols, Department of Geology, Tufts University, Medford, Mass.

G–Botanical Sciences. (Probably; in cooperation with botanical societies.) Dr. Barry Commoner, Henry Shaw School of Botany, Washington University, St. Louis 5, Mo.

H–Anthropology. Dr. Gabriel Lasker, Wayne University College of Medicine, 1401 Rivard Street, Detroit 7, Mich.

I–Psychology. Dr. Conrad G. Mueller, Department of Psychology, Columbia University, New York 27, N.Y.

L–History and Philosophy of Science. Dr. Jane M. Oppenheimer, Department of Biology, Bryn Mawr College, Bryn Mawr, Pa.

Nd–Dentistry. Dr. George C. Paffenbarger, American Dental Association Research Fellowship, National Bureau of Standards, Washington 25, D.C.

Np–Pharmacy. Dr. John E. Christian, School of Pharmacy, Purdue University, Lafayette, Ind.

Q–Education. Dr. Herbert A. Smith, 205 Bailey, School of Education, University of Kansas, Lawrence, Kan.

BOOK REVIEWS

The Viking Rocket Story. Milton W. Rosen. Harper, New York, 1955. 242 pp. Illus. + plates. $3.75.

This book is an account of the development of the Viking rocket, an American vehicle specifically designed for the exploration of the upper atmosphere. The description of the 11 rocket firings, of which the last reached a peak altitude of 158 miles with a payload of 825 pounds of scientific instruments, should serve as an antidote to the flood of escapist technical literature outlining in broad strokes of the pen schemes for manned flight to neighboring astronomical bodies. Referring to these excursions of the fancy, Rosen writes: "It looked so easy when you did it on paper—where valves never froze, gyros never drifted, and rocket motors did not blow up in your face." In this book the reader is presented with a dynamic account of the activities in the White Sands rocket launching area leading to the firing of these high-altitude vehicles and an analysis of the mechanical and human factors that make a successful ascent even to the first 100 miles not always predictable.

The early chapters of the book contain a historical survey of rocketing covering the work of Hermann Oberth in Germany and Goddard in America. This is followed by a description of the principles of rocket propulsion, placing emphasis on the unique gimbaled construction of the Viking motor, which permits steering of the vehicle. Interwoven among the series of rocket firings is a description of methods for telemetering information during the flight of the vehicle to ground receiving stations, methods for the physical recovery of scientific equipment carrying photographic records of the solar spectrum and cosmic radiation, and methods for measuring the altitude and velocity of the rocket by means of optical, radar, and Doppler tracking. The laboratory investigator contemplating exposures of material to cosmic radiation at high altitudes will find this information invaluable in planning experiments.

The book is liberally illustrated with photographs of the earth taken from high altitude and the tracks of heavy cosmic-ray particles recorded in nuclear emulsions during the flight of Viking rockets. The scenes of the launching area, rocket takeoffs, and equipment recovery are helpful in providing the reader with a vicarious feeling of participating in the firings. Some of the illustrations recall scenes from the Old Testament. The activities around the launching gantry during the fueling operation bring to mind the Tower of Babel, and one wonders whether the scientists scurrying about the 42-foot vehicle have prayers in their hearts that the Lord will not confound their telemetering channels.

The book abounds in characterizations of the men associated with the Viking project and exhibits a fine literary feeling for the arid White Sands desert region rarely encountered in a work of a technical nature: "The New Mexico sandstorm is an implacable enemy of rocket work. You can see it coming in the distance, a long whitish cloud crawling close to the ground. It strikes with unbelievable force, sometimes as a steady gale, sometimes in gusts, sandblasting everything in its path. . . . When a storm approached the men would rush to pull the rubberized covers over the rocket and would lash it to the gantry structure. Then there was nothing to do but wait the storm out."

In this respect Rosen's prose is reminiscent of Antoine de Saint Exupéry's classic, *Wind, Sand and Stars,* dealing with the early days of aviation when the cloth airplane wings were strengthened with a film of lacquer. Rosen has made a valuable contribution to the literature on high-altitude rocketry which will be read with interest and profit both by the scientist and the layman interested in the new frontier.

HERMAN YAGODA
National Institutes of Health

Culture and Experience. A. Irving Hallowell. University of Pennsylvania Press, Philadelphia, 1955. xvi + 434 pp. $7.

This volume brings together a well-chosen selection of A. I. Hallowell's published papers, with a few new sections that either supplement or serve to introduce and integrate the themes of the other chapters. It meets a more definite need than do typical publications of this sort, because the broad and interdisciplinary interests of the author have led him to publish articles in diverse journals, which are not all readily accessible to any one reader.

As the work of an anthropologist who has contributed a great deal to the developing interrelationship of anthropology and psychology, the book will be of greatest interest to members of these two disciplines and to others especially interested in their relationship. Hallowell is perhaps best known to psychololgists through his pioneering in the addition of the Rorschach personality test to the field techniques of anthropology, and this activity is well represented here by an up-to-date review of its outcome.

More significant in my opinion, and here gathered together for the first time, are Hallowell's applications to a number of psychological topics of his thorough knowledge of Ojibwa culture. His intimate acquaintance with this, as an example of a culture that differs greatly from ours, is drawn on for consideration of how man develops his concepts of time, space, and measurement, illustrating in each case the variation of such concepts with the cultural setting. The nature of property is a concept of different order similarly analyzed. Anxiety, aggression, and psychosexual adjustment are topics in motivational psychology on which also this penetrating light of cultural diversity is cast in successive chapters.

In all this, Hallowell is concerned with advancing understanding on all fronts, not with persuading the reader to accept some one emphasis or point of view. Most stimulating of all through present novelty are the

very recent papers considering the concept of self or ego in the light of cross-cultural comparisons.

The volume is not restricted to the boundary between anthropology and psychology. A section on acculturation bears directly on sociological problems as well, and the relevance of biology is considered in a chapter on the recapitulation theory and at appropriate points elsewhere. In all, the book is an admirable instance of the considerable progress that has been made toward breaking the interdisciplinary barriers of technique and construct at points where they hamper and thus achieving a general science of behavior.

IRVIN L. CHILD
Department of Psychology, Yale University

Science and Man's Hope. James Street Fulton. Bookman Associates, New York, 1955. 179 pp. $3.25.

Scientists as a class have not been notably distinguished for, or interested in, the metaphysical implications of their activities. Most of them would, I presume, claim that their scientific preoccupation left little or no time for philosophy. But with the phenomenal success of science in recent generations, we can no longer afford to ignore its effect upon our way of life or its susceptibility in our culture of being used by malevolent power. Whether it is this or other factors that are responsible, one cannot help being increasingly aware of the growing concern with the problem of science's place in the nature of things.

James Fulton in *Science and Man's Hope* is occupied fundamentally with the definition of the good life, which he identifies in part with the value of life and living, an openness to life, a compassion for others, a heightened sensitivity and with love. The core of his exposition is the existence and identification of these values, and he devotes a major portion of his book to demonstrating that science has nothing to do with either recognizing or establishing them. Science, he insists, can only discover facts; it cannot impart value to them.

Much of what Fulton has to say possesses cogency, even though its expression is frequently difficult and unnecessarily elliptical.

HARRY L. SHAPIRO
Department of Anthropology,
American Museum of Natural History

Man's Emerging Mind. Man's progress through time—trees, ice, flood, atoms and the universe. N. J. Berrill. Dodd, Mead, New York, 1955. 308 pp. $4.

Fundamentally, this book recounts the familiar story of evolution as it concerns the mammals, biased to consider mostly the primates and man's own history. Secondarily, it weaves in by means of the first person singular the philosophy of "I, John Berrill, a selfconscious fragment of life." The weaving is skillful, knowledgeable, scientific. The product is not a pattern in two dimensions but a panoramic view of man's past spread 3-D on the scale of time. Throughout an attempt is made to peek around the corner and guess at possibilities in the future as extrapolations of what has already taken place. This thought of the future is personal too, with numerous comments on the relative merits of burial, mummification, and cremation.

Repeatedly the reader is introduced to the emergent concept of evolution and shown how each change in the ancestral line—whether structural, physiological, or sociological—permitted development in directions that had not yet become apparent when the change occurred. Berrill regards mankind's present problem of overpopulation as the final step on a growth plateau made possible by the invention of agriculture. He is hopeful that a way to scale new heights will be found but insists that "the essence of man is his quality, not his quantity, and this is no place to stop, halfway between ape and angel."

The decoration on the jacket stems from this line. It is a pity that the artist was not allowed to add enlivening caricatures throughout the text. Three dozen references "For further reading," and a brief index complete the book.

LORUS J. AND MARGERY MILNE
Department of Zoology, University of New Hampshire

Culture and Mental Disorders. Joseph W. Eaton and Robert J. Weil. Free Press, Glencoe, Ill., 1955. 254 pp. $4.

The scientific approach to man and his problems is a 20th-century phenomenon. During the first three decades, stimulated primarily by Freud, scientific insight into man's inner emotional turmoil, or psychodynamics, was intensively developed. Since then we have become increasingly aware of social and cultural influences that affect the adaptation of any human being. A good part of the progress in the scientific understanding of man's behavior has been achieved at the expense of neglecting or denying any inherent or genetic influences. It is as if on finding how misled they were by overzealous 19th-century biologists, behavioral scientists preferred to do without nature and stake their all on nurture. The publication of *Culture and Mental Disorders* by Eaton and Weil points up how far we have traveled in this 20th century. For here, in a calm and deliberate fashion, the emotions, social life, and constitutional factors are given due regard in a carefully planned study of mental illness.

The authors begin their research within a framework of three rather conflicting lines of argument. Psychologically, personal adaptation is the result of the internalization of early experiences whose emotional meaning provides generalizations that affect all future emotional responses. The consistency of this inner life is considered to be the basis of "good" mental health. Sociologically, it is in the experiences themselves that conflicts develop, and therefore social organization and the degree of consistency of cultural norms provide the basis for healthy adjustment. Biologically, it is understood

that any organism has by accident or genetic history a differential ability to adapt to stress, whether inner or outer. To test or synthesize these approaches, the authors of the book under review took an intensive look at the mental health of the Hutterites, a fundamentalist religious group that, although it exists in various colonies in the United States and Canada, manages to keep itself isolated and has a remarkably integrated and smoothly running social organization. They compare their findings with those of nine other intensive surveys of mental health. In order to develop these comparisons, some new and highly useful techniques for calculating rates and incidence have been developed.

The results of the survey and the comparisons indicate that, although no society avoids mental illness, the over-all rates will vary somewhat and the degree of severity will vary in terms of the stresses that produce the breakdown *and* the stress the breakdown produces within the culture. Most interesting is the generalization that, in well-integrated social systems, the disorder most likely to appear is a so-called "manic-depressive psychosis," whereas, in more individualistic societies, schizophrenia is the more frequently diagnosed disorder. Perhaps equally important, although it is an aspect that receives less attention in this monograph, is the finding that on projective tests the Hutterites show considerable inner turmoil and a potentiality for many severe antisocial acts, but these tendencies do not get acted out. Being set on maintaining social equilibrium, the Hutterites are neither creative nor in the Western civilized sense progressive. Evidently, the price we pay for individualism, search, and change may consist not of greatly increased rates of mental disorder but only of more severe mental breakdowns of a special type. Although inherent potentialities for breakdown exist to some extent in all individuals, it is stability that is the stress for some and complexity that is the stress for others.

These are the important results of the Eaton and Weil study. The research methods, the successful use of past researches, and integration of the disciplines of the behavioral sciences make this well-written book an important contribution to our present knowledge and a powerful stimulant for further work.

MARTIN B. LOEB
School of Education, University of Kansas City

Scientific Writing. Meta R. Emberger and Marian R. Hall. Harcourt, Brace, New York, 1955. xii + 468 pp. Illus. $4.50.

Scientific Writing begins its exposition with the intellectual activity that precedes writing, goes through the collection of data, the analysis of the results, and finally their interpretation. Another chapter then considers readers. This is very important, since scientific writing is at present not only directed toward other scientists but is also addressed to administrators, policy makers, and, to some extent, the general public.

Chapters on the actual writing of a scientific paper include the development of scientific style and the techniques of exposition. The types of scientific papers are then discussed separately. These include research papers, short and long reports, abstracts, case histories, and book reviews.

There is an excellent chapter on format and documentation and another dealing with graphic and pictorial illustration. Appendix A provides selected readings and word lists, and Appendix B gives samples of letters designed to accompany the formal paper, report, or grant request. The letters are carefully planned to introduce the material to the recipient, whether this be editor, supervisor, or a granting agency.

The book should be of considerable use to scientists in meeting the problems presented by the communication of their findings to others. Although its textbook style does not lend itself easily to consecutive reading, it should prove to be very helpful in a course of instruction on scientific writing, and research workers will find it a valuable reference tool for specific problems encountered in this sphere. The organization of the book is good, and the inclusion of so many important phases of scientific writing makes it particularly useful as a guide for the author, editor, and reviewer of scientific literature. The variety of examples and the excellent illustrations in the graphic portion give it a particular appeal for me.

RAYMUND L. ZWEMER
Natural Sciences Department, UNESCO, Paris

Treatise on Invertebrate Paleontology. Part E, *Archaeocyatha and Porifera.* Prepared under the guidance of the Joint Committee on Invertebrate Paleontology. Raymond C. Moore, Ed. Univ. of Kansas Press, Lawrence; Geological Society of America, New York 27, 1955. xviii + 122 pp. Illus. $3.

The purpose of the *Treatise on Invertebrate Paleontology* is "to present the most comprehensive and authoritative, yet compact statement of knowledge concerning invertebrate fossil groups that can be formulated by collaboration of competent specialists in seeking to organize what has been learned of this subject up to the mid-point of the present century." Nearly 150 specialists, representing many countries will contribute to the *Treatise,* which is prepared under the guidance of the Joint Committee on Invertebrate Paleontology, representing the Paleontological Society, the Society of Economic Paleontologists, and the Palaeontographical Society (of Great Britian). Two parts of the *Treatise* have previously appeared: Part G, *Bryozoa* and part D, *Protista* 3 (radiolarians, tintinnines). The present volume, dealing with fossil sponges and certain extinct spongelike organisms, is a welcome addition to this badly needed compendium of morphologic and taxonomic knowledge.

The first section of this volume, by Vladimir J. Okulitch, describes and classifies 75 genera of the Archaeocyatha (referred to in some recent publications as the Pleospongia), an extinct group of organisms that has been variously placed among the algae, the protozoans,

coelenterates, and sponges. Agreeing with current opinion that the archaeocyathids cannot be considered as algae, protozoans, or coelenterates, Okulitch summarizes morphological and stratigraphic evidence that the group in question should not be classed with the sponges and concludes that it deserves to rank as a separate phylum. This judgment has considerable theoretical interest, since it renders archaeocyathids the only extinct group of organisms now seriously regarded as a phylum.

The second part of this volume, by M. W. de Laubenfels, illustrates and characterizes 778 undoubted genera of fossil sponges and deals briefly with sponge physiology and ecology. Eleven genera of receptaculitids, a group of spongelike fossils whose biological affinities have puzzled generations of paleontologists, are described and (rather inadequately) illustrated. Their taxonomic status remains in doubt.

Although the record of both sponges and the morphologically similar archaeocyathids begins in the Early Cambrian, the two groups have strikingly different histories. The taxonomic diversity of the archaeocyathids (as judged from the number of families) decreased sharply by Middle Cambrian times, and no members of that group are known from younger strata. Judging from de Laubenfels' data, however, sponges were more diverse in Ordovician than in Cambrian times. Is this an instance of ecological replacement involving whole phyla? Simpson, arguing that archaeocyathids do not merit consideration as a phylum separate from the Porifera (*The Meaning of Evolution,* 1949, p. 37), has concluded that ecological replacement on this scale is unknown (*The Major Features of Evolution,* 1953, p. 172). One of the fruits of this volume, and of other parts of the *Treatise,* will be the opportunity for students in diverse disciplines to examine some of the basic data bearing on these and related questions of general evolutionary interest.

John Imbrie
Department of Geology, Columbia University

The Life and Work of Sigmund Freud. *Years of Maturity, 1901–1919.* vol. 2. Ernest Jones. Basic Books, New York, 1955. xiii + 512 pp. plates. $6.75.

It would have been hard to maintain the high standard of interest set by the first volume of his definitive biography of Freud, and Ernest Jones has not quite been able to turn the trick. There are no exciting disclosures in this second volume, and the only dramatic events in Freud's life during the first two decades of our century were afforded by the rise of psychoanalysis into public prominence, the growth of the movement, and certain dissensions within it. These matters are presented in frequently vivid detail, excellent use being made of quotations from Freud's extensive correspondence.

Almost half the book is devoted to conscientious summaries and discussions of Freud's publications. They will be of value primarily to the many readers who have read a few of Freud's major works and understand enough of the theory to be interested in a chronological summary of everything that he wrote during this time. Jones is a better summarizer than critic. His organization of the material brings out its many-sided breadth of contribution—to therapeutic technique, clinical fact, theory, and various applications to other fields. But the serious student cannot look to Jones for expert guidance in sifting the wheat from the chaff in Freud's tremendous output.

This volume may be of most lasting scientific value for the insights it gives into the peculiar conditions shaping the development of psychoanalysis. Never letting us forget that he was a mover and shaker himself, Jones recreates the intimate atmosphere of the international psychoanalytic movement during these years —something between a group of disciples around a great master, a gentleman's intellectual club, and a small but militant and persecuted band of crusaders bound together by a network of personal relationships and loyalties. Knowing this, we can perhaps better understand the special strengths and deficiencies of psychoanalysis as a science.

Robert R. Holt
Research Center for Mental Health, New York University

Principles of Mathematics. C. B. Allendoerfer and C. O. Oakley. McGraw-Hill, New York, 1955. xv + 448 pp. Illus. $5.

About 1916 an innovation appeared in first-year college mathematics when a textbook integrated algebra, analytic geometry, and calculus into a year of continuous study. Now, in 1955, after 5 years of classroom trial and revision, another textbook not only maintains the integrative idea but also introduces content and method of modern mathematics into the first-year course. In the first five chapters, there are presented the most elementary ideas of symbolic logic, number systems, groups, fields, sets, and Boolian algebra. The remaining chapters are traditional only in name, for the treatment of function, analytic geometry, limits, the calculus, probability, and statistics has a modern flavor, is in harmony with contemporary mathematical thought, and completely reoriented away from the traditional viewpoint.

For an acceptable treatment of so many topics in one book designed for 1 year of study, there had to be much elimination of the usual subject matter of first-year college study. Many common topics of college algebra and analytic geometry are absent. Frequently reference is made to the fact that further treatment is beyond the scope of this book and must be pursued in later courses. Much of usual textual development of formulas and laws is posed as exercises or left for the instructor's own presentation. The aim of the authors is to give students insight into the methods and content of present-day mathematics through a number of relatively brief treatments of modern topics rather than to give a thorough and more complete treatment of only a few topics.

The concepts of mathematics are stressed, both in

the development and in the exercises. Practice in thinking is far more prevalent than practice in manipulation of the symbols. In addition, appended to each chapter is a list of assignable readings in the current literature. A check of these references assured me that they are well within the comprehension of good first-year college students. As a further motivation, answers to selected problems are given in the appendix.

Several features are worthy of special mention. The list of symbols on pages xiv-xv will convince the student that he has in store a new study of mathematics entirely different from his secondary-school study. The development and use of this symbolism is gradual and temperate, so as not to discourage a student who has not yet developed a taste for axioms and abstractions. In fact, one criticism may well be that not enough use is made of new concepts and symbolisms in the development of the later chapters. The development of function as an ordered pair, formed by mapping, and the distinction between relation and function are fine pieces of pedagogy. The fusion of probability, combinations, and statistics in one chapter has produced a most satisfying introduction to an area of applied mathematics.

Certain questions of feasibility will arise in the mind of one who examines this book. Why introduce group, field, and Boolian algebra, and then, with one exception, never use them again? Why such a rigorous treatment of the calculus in such condensed form (34 pages for eight lessons, including differentiation, integration, the proof of the fundamental theorem, and application)? Defining complex numbers as ordered pairs, with no previous study of this manner of developing number, to say i is not a number, and then $i^2 = -1$ (which certainly is a number), must be confusing to first-year students.

Students who master this book will be in command of a store of concepts that, although in themselves are of little use, will prove invaluable for subsequent study of mathematics.

HOWARD F. FEHR
Department of Teaching of Mathematics,
Teachers College, Columbia University

The Sun and Its Influence. An introduction to the study of solar-terrestrial relations. M. A. Ellison. Macmillan, New York, 1955. 235 pp. $4.50.

This book is written for "those with a knowledge of elementary physics and for workers in other sciences." I am afraid that the author has not succeeded in his aim to write for such a wide audience. Although the discussion is mostly in general terms, the author has not overcome the difficulty of avoiding both the Charybdis of explaining too much (for instance, footnotes explain what is meant by angstrom, degrees Kelvin, and microsecond) and the Scylla of taking too much knowledge for granted (for instance, Baade's types I and II are mentioned without explanation). This is, however, a minor point, and it would be ungrateful to stress it too much, since the book unfolds a broad and very pleasing picture of solar physics and its influence on terrestrial phenomena, especially its influence on radio phenomena. The author is clearly an expert in this field and discourses with obvious pleasure on his own subject.

The book discusses the following topics: solar radiation, solar activity (sunspots), the solar atmosphere (prominences and corona), the ionosphere, solar flares, the sun's and the earth's magnetism, the aurora, radio waves from the sun, and cosmic rays. This book can be highly recommended to those readers who have a reasonable knowledge of physics and radio. It is well written and presents a stimulating picture of advances in a relatively young branch of science.

D. TER HAAR
Clarendon Laboratory, Oxford

Standing Room Only. The challenge of overpopulation. Karl Sax. Beacon Press, Boston, 1955. 206 pp. $3.

This is a hard-hitting book set forth in easy reading style and it is rapidly paced. It deals with the factors that have led to the present unprecedented growth of world population, considered in relation to agriculture, industry, and medicine. It draws from a prodigious background of statistical information and gives only the figures needed to make pertinent points. Tables and graphs are easy to comprehend, and each seems to pack an unusually stiff punch.

The book opens with the statement that promises of an abundant life, of freedom from want, for all of mankind are usually made with little or no consideration of population trends. Attention is called to the fact that, while people of the modern industrial nations and the economically underdeveloped countries of the world are now aware of the possibilities of human advancement through the application of newer scientific knowledge, few are aware of the problems and difficulties that must be solved if all of the world's people are to escape from hunger and poverty. With much supporting evidence, the assertion is made that, unless the new frontiers of science can develop abundant new sources of energy and essential minerals, the modern industrial culture will be but a brief episode in human history.

The problem of population, it emphasizes, is made more difficult by the fact that the greatest growth, or greatest potential growth, is in the underdeveloped areas of the world where population pressure already is acute and living standards are low. These areas are described as not yet having made the transition from a high-birth-rate, high-death-rate culture with low living standards to a low-birth-rate, low-death-rate one with higher living standards. Based on well-substantiated trends, a general picture of progressive economic and cultural development is set forth, being built around the idea of "demographic transition."

Modern Western nations comprising only about 15 percent of the world's population, it is pointed out, have in effect completed the demographic transition and, in doing so, have depleted their capital resources

at an alarming rate. Question is raised then as to the likelihood of the rest of the world making a similar transition. The body of evidence brought forth indicates this to be essentially an impossible task unless birth rates are materially reduced. Attention is given to the obstacles of tradition and to the conflict between creeds and needs, characterizing the resistance to change. Throughout the book, attention is drawn to the necessity for rational human behavior if satisfying progress is to be made, but emphasis is placed on the fact that human behavior is so very often not significantly rational.

At the end, it is stated that mankind has the knowledge and potential resources to provide a good life for all the world's people if population growth can be controlled, and that modern man is in a position to choose either a future based on ignorance and superstition or on science and rational thought. The final conclusion is that in the future, as in the past, population growth will be controlled by wars, famine, and disease unless birth rates in all parts of the world are soon adjusted to moderate levels.

PAUL S. HENSHAW
National Committee on Maternal Health

Antimetabolites and Cancer. A symposium. Cornelius P. Rhoads, Ed. American Association for the Advancement of Science, Washington, D. C., 1955. vi + 312 pp. Illus. $5.75; AAAS members, $5.

This volume records a 2-day symposium sponsored by the AAAS in December 1953 and represents a rather comprehensive review of the status of the "antimetabolite approach" to neoplastic disease as of that date. The 18 contributors elaborate on the applications of this approach to the cancer phenomenon, from the perspective of a wide variety of experimental designs. These range from the plant studies of L. G. Nickell, and the studies of the protozoan *Tetrahymena* (R. E. Parks), bacterial growth and differentiation (B. D. Davis, D. W. Visser, D. W. Woolley), through the wide variety of experimental mammalian tumors (E. M. Lansford and W. Shive, J. R. Totter, Wooley, H. G. Mandel, H. E. Skipper, G. H. Hitchings) to a consideration of clinical data obtained in human leukemias (J. H. Burchenal).

At the time of this symposium and, to be sure, at the present moment, the concept of metabolic blockade as the rational approach to the ultimate goal of developing one or many effective antihuman-tumor agents, represents the direction of greatest current promise. C. P. Rhoads indicates with conviction the lines of experimental interpretation that support this view. If a metabolic Achilles-heel is to be found for "the cancer cell," the logical first order of business should be concerned with the detection of the point of metabolic departure of the neoplastic cell from its normal counterparts. To date, no such deviation has been discerned. Thus, the approach of "enlightened empiricism" has been adopted in the majority of laboratories as the "next best" line of attack.

Reappraising the evidence of Warburg (1930) that indicated a high rate of aerobic glycolysis in neoplastic cells, Weinhouse cites more recent studies showing that cancer cells utilize glucose and fatty acids as their major metabolites along the same metabolic pathways as normal cells, and that the high rate of glycolysis via the fructose-1-6-diphosphate "Embden-Myerhof cycle" is a supplementary, rather than a replacement, mechanism.

The preeminence of the role of nucleic acids in the cancer cell has directed the line of investigation of the majority of these contributors. Hitchings summarizes current knowledge of the probable sites of action of the folic acid and purine antagonists in the synthesis of the purines and pyrimidines. Along these lines, Woolley describes his studies of the vitamin B_{12} precursor, 1,2-dimethyl, 4,5-diaminobenzene and the antimetabolic effects of its chemical analogs in spontaneous and transplanted mouse mammary cancers. The differences in sensitivity to these analogs appeared to be related to the differences between host and tumor tissue in ability to synthesize B_{12}.

The importance of specific alternate metabolic pathways in the synthesis of nucleic acids in the determination of specific tumor resistance or sensitivity to a particular antimetabolite is discussed by Skipper, with particular reference to 8-azaguanine and α-methopterin. Burchenal's report on the progress of therapy in acute leukemia since the introduction of the folic acid and purine antagonists constitutes one of the more optimistic notes in the application of antimetabolic agents to human cancer.

The book represents a valuable summary and is well notated bibliographically.

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Marine Shells of the Western Coast of Florida. With revisions and additions to Louise M. Perry's *Marine Shells of the Southwest Coast of Florida.* Louise M. Perry and Jeanne S. Schwengel. Paleontological Research Institution, Ithaca, N.Y., 1955. 318 pp. Illus. Paper, $6., cloth, $7.

When the first edition of this book came out in 1940 the author, Louise M. Perry, called it *Marine Shells of the Southwest Coast of Florida,* because she had spent many winters in this part of Florida, making Sanibel Island, southwest of Fort Myers, her residence. Here she gathered and studied the marine mollusks of the area and helped spread the fame of Sanibel as the choicest shell-collecting spot in the country. Perry assiduously watched in her study the living mollusks that she brought home from her excursions and made careful notes on their habits, especially on the eggs they laid.

These valuable notes, which were not included in the first edition, have now been added by Jeanne Schwengel, who has also arranged to make use of Margaret Storey's drawings of eggs, egg capsules, and larvae, made under Perry's supervision. The text includes notes on the eggs and egg capsules of 19 species, of which 13 are based on personal observations and others are quoted from the

works of Marie Lebour of England. The eggs and egg capsules of 24 species are illustrated. Twenty-three species not in the first edition are included in this book, while eight species are omitted here for one reason or another. Sixteen new plates have been added.

The nomenclature is up to date, and I have found no obvious misidentifications and only a few typographic errors. The only suggestions I would make would be that on page 55 *Modiolaria* Beck, 1838 (not "1938") should be called *Musculus* Röding, 1798, and on page 63 the name *Taras* Risso, 1826, is now generally considered a *nomen dubium* and is replaced by *Diplodonta* Bronn (not "Brown"), 1831. Similarly, the group called *Gastrochaena* Spengler, 1783, on page 93, is now generally known as *Rocellaria* Blainville, 1829. The index has been materially improved by including all the specific names in alphabetical order rather than placing them under the alphabetically arranged generic names.

Louise Perry and Jeanne Schwengel started as amateurs to collect shells as a hobby and, through increasing interest and study, reached a high degree of scientific competence and reputation. Their book shows how the amateur collector can add greatly to our knowledge in malacology. Much still remains to be discovered about the life-history and habits, especially the egg-laying methods, of even our common species of mollusks. The facts discovered by careful observation in the field and in the home aquarium, for which generally the systematic worker and museum professional have too little time, have often a direct bearing on and importance in the proper classification of the mollusks.

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2 March

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The Physiology of Diapause in Arthropods, A. D. Lees (Cambridge University Press). Reviewed by H. A. Schneiderman.

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Proceedings of the Thirty-Fourth Annual Meeting, Highway Research Board. Fred Burggraf, Elmer M. Ward and Walter J. Miller, Eds. National Acad. of Sciences–National Research Council, Washington, 1955. 648 pp. $8.50.

In Quest of Knowledge. A historical perspective on adult education. C. Hartley Grattan. Association Press, New York, 1955. 337 pp. $4.75.

Glossary of Packaging Terms. Standard definitions of trade terms commonly used in packaging. Packaging Inst., New York 17, ed. 2, 1955. 323 pp. $6.75.

Stories of Scientific Imagination. Joseph Gallant, Ed. Oxford, New York 3, 1954. 152 pp. Paper, $0.50; cloth, $0.85.

Essays in Today's Science. Joseph Gallant, Ed. Oxford, New York, 1955. 150 pp. Paper, $0.50; cloth, $0.85.

Structural Geology for Petroleum Geologists. William L. Russell. McGraw-Hill, New York, 1955. 427 pp. $7.50.

Analytic Geometry. Clyde E. Love and Earl D. Rainville. Macmillan, New York, ed. 5, 1955. 302 pp. $4.

A Textbook of Sound. Being an account of the physics of vibrations with special reference to recent theoretical and technical developments. A. B. Wood. Macmillan, New York, 1955. 610 pp. $6.75.

Human Relations in Interracial Housing. A study of the contact hypothesis. Daniel M. Wilner, Rosabelle Price Walkley, and Stuart W. Cook. Univ. of Minnesota Press, Minneapolis, 1955. 167 pp. $4.

Mass-Transfer Operations. Robert E. Treybal. McGraw-Hill, New York, 1955. 666 pp. $9.50.

Cellulose and Cellulose Derivatives. pt. III. Emil Ott, Harold M. Spurlin, and Mildred W. Grafflin, Eds. Interscience, New York–London, rev. and augmented ed. 2, 1955. 543 pp. $12.

The Alkaloids, Chemistry and Physiology. vol. V, *Pharmacology*. R. H. F. Manske, Ed. Academic Press, New York, 1955. 388 pp. $9.50.

Topological Transformation Groups. Deane Montgomery and Leo Zippin. Interscience, New York–London, 1955. 282 pp. $5.50.

Topley and Wilson's Principles of Bacteriology and Immunity. 2 vols. G. S. Wilson and A. A. Miles. Williams & Wilkins, Baltimore, ed. 4, 1955. 2331 pp. $24.50.

Trigonometrical Series. Antoni Zygmund. Dover, New York, 1955. 329 pp.

Problems in Amoebiasis. Charles William Rees. Thomas, Springfield, Ill., 1955. 119 pp.

General Chemistry for Colleges. B. Smith Hopkins and John C. Bailar, Jr. Heath, Boston, ed. 5, 1956. 701 pp. $6.

Plastics Progress, 1955. Papers and discussions at the British Plastics Convention, 1955. Phillip Morgan, Ed. Iliffe, London; Philosophical Library, New York, 1956. 432 pp. $17.50.

Introductory Quantitative Chemistry. Axel R. Olson, Charles W. Koch, and George C. Pimentel. Freeman, San Francisco, 1956. 470 pp. $5.

Meetings

June

3–6. American Soc. of Refrigerating Engineers, Cincinnati, Ohio. (R. C. Cross, ASRE, 234 Fifth Ave., New York 1.)

3–7. Special Libraries Assoc., annual, Pittsburgh, Pa. (Miss M. E. Lucius, 31 E. 10 St., New York 3.)

3–8. Soc. of Automotive Engineers, summer meeting, Atlantic City, N.J. (Meetings Div., SAE, 29 W. 39 St., New York 18.)

4–7. Forest Products Research Soc., Asheville, N.C. (F. J. Rovsek, FPRS, P.O. Box 2010, University Station, Madison 5, Wis.)

4–9. International Mechanical Engineering Corp., 6th, Paris, France. (British Engineers Assoc., 32 Victoria St., London, S.W.1, England.)

4–9. International Seed Testing Convention, Paris, France. (C. Stahl, International Seed Testing Assoc., Thorvaldsensvej 57, Copenhagen V, Denmark.)

4–9. Microbiological Inst., 9th annual, Lafayette, Ind. (Div. of Adult Education, Engineering Administration Bldg., Purdue Univ., Lafayette.)

6–8. American Soc. for Quality Control, annual, Montreal, Quebec, Canada. (C. E. Fisher, ASQC, 50 Church St., Room 563, New York 7.)

6–9. European Federation for Chemical Engineering, 9th, Frankfurt/Main, Germany. (Dechema-Haus, Rheingau-Allee 25, Frankfurt A.M.)

6–10. American College of Chest Physicians, annual, Chicago, Ill. (M. Kornfeld, ACCP, 112 E. Chestnut St., Chicago.)

7–9. Endocrine Soc., annual, Chicago, Ill. (H. H. Turner, 1200 N. Walker St., Oklahoma City, Okla.)

9–10. Soc. for Investigative Dermatology, annual, Chicago, Ill. (H. Beerman, 255 S. 17 St., Philadelphia 3, Pa.)

10–14. Institute of Food Technologists, annual, St. Louis, Mo. (C. S. Lawrence, IFT, 176 W. Adams St., Chicago 3, Ill.)

10–15. American Crystallographic Assoc., French Lick, Ind. (S. Siegel, Chemistry Div., Argonne National Lab., Box 299, Lemont, Ill.)

11–15. American Medical Assoc., annual, Chicago, Ill. (G. F. Lull, AMA, 535 N. Dearborn St., Chicago 10.)

11–15. Symposium on Molecular Structure and Spectroscopy, annual, Columbus, Ohio. (H. H. Nielsen, Dept. of Physics, Ohio State Univ., Columbus.)

11–16. Pacific Div., AAAS, Seattle, Wash. (R. C. Miller, California Acad. of Sciences, Golden Gate Park, San Francisco 18.)

11–23. European Organization for Nuclear Research, Symposium on High Energy Physics, Geneva, Switzerland. (H. Coblans, CERN, Case Postale 25, Genève 15-Aéroport.)

12–14. American Meteorological Soc., Seattle, Wash. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

12–15. Max Planck Soc. for Advancement of Sciences, Stuttgart, Germany. (Max Planck Soc., Raiserwertherstrasse 164, Düsseldorf 22a, Germany.)

12–16. World Conf. on Earthquake Engineering, Berkeley, Calif. (R. W. Clough, Div. of Civil Engineering, Univ. of California, Berkeley 4.)

13–14. Conf. for Veterinarians, 25th annual, Columbus, Ohio. (J. W. Helwig, College of Veterinary Medicine, Ohio State Univ., Columbus, 10.)

13–16. Colloquium of College Physicists, annual, Iowa City, Iowa. (G. W. Stewart, Dept. of Physics, State Univ. of Iowa, Iowa City.)

17–20. American Soc. of Agricultural Engineers, 49th annual, Roanoke, Va. (F. B. Lanham, ASAE, St. Joseph, Mich.)

17–22. American Soc. of Medical Technologists, annual, Quebec, Canada. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

17–22. American Soc. for Testing Materials, annual, Atlantic City, N.J. (R. J. Painter, ASTM, 1916 Race St., Philadelphia 3, Pa.)

17–23. American Library Assoc., annual, Miami Beach, Fla. (D. H. Clift, 50 E. Huron St., Chicago 11, Ill.)

17–23. World Confederation for Physical Therapy, 2nd international cong., New York, N.Y. (Miss M. Elson, American Physical Therapy Assoc., 1790 Broadway, New York 19.)

17–23. World Power Conf. (invitational), 5th plenary, Vienna, Austria. (S. E. Reimel, Engineers Joint Council, 29 W. 39 St., New York 18.)

17–30. West Coast Science Teachers Summer Conf., Corvallis, Oreg. (R. H. Carleton, National Science Teachers Assoc., 1201 16 St., NW, Washington 6.)

18–20. American Soc. of Heating and Air-Conditioning Engineers, Washington, D.C. (A. V. Hutchinson, ASHAE, 62 Worth St., New York 13.)

18–21. Institute of Aeronautical Sciences, Inc., annual summer, Los Angeles, Calif. (S. P. Johnston, 2 E. 64 St., New York 21.)

18–21. Phi Lambda Upsilon, triennium convention, Ann Arbor, Mich. (T. B. Cameron, Dept. of Chemistry, Univ. of Cincinnati, Cincinnati 21, Ohio.)

18–22. American Physical Therapy Assoc., annual, New York, N.Y. (Miss M. Elson, APTA, 1790 Broadway, New York 19.)

18–22. Medical Library Assoc., 55th annual, Los Angeles, Calif. (A. N. Brandon, Library, College of Medical Evangelists, Loma Linda, Calif.)

18–24. Acoustical Soc. of America, Cambridge, Mass. (W. Waterfall, ASA, 57 E. 55 St., New York 22.)

19–22. American Dairy Science Assoc., annual, Storrs, Conn. (H. F. Judkins, 32 Ridgeway Circle, White Plains, N.Y.)

20–22. American Assoc. of Physics Teachers, annual, Toronto, Ont., Canada. (F. Verbrugge, Carleton College, Northfield, Minn.)

20–28. International Union for the Protection of Nature, Edinburgh, Scotland. (J. P. Harroy, IUPN, 42, rue Montoyer, Brussels, Belgium.)

21–23. Soc. of Nuclear Medicine, annual, Salt Lake City, Utah. (R. G. Moffat, 2656 Heather St., Vancouver 9, Canada.)

21–24. American Acad. of Dental Medicine, 10th annual, Detroit, Mich. (G. J. Witkin, AADM, 45 S. Broadway, Yonkers 2, N.Y.)

21–24. American Soc. of Ichthyologists and Herpetologists, 36th annual, Higgins Lake, Mich. (R. M. Bailey, Museum of Zoology, University of Michigan, Ann Arbor.)

25–27. Symposium on Uses of High Temperatures in Science and Industry, Berkeley, Calif. (N. K. Hiester, Stanford Research Inst., Menlo Park, Calif.)

25–29. American Soc. for Engineering Education, annual, Ames, Iowa. (W. Leighton Collins, Univ. of Illinois, Urbana.)